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A
JOURNAL
OF
NATURAL PHILOSOPHY,
CHEMISTRY,
AND
THE ARTS.

VOL. XVI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:
PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR
THE AUTHOR,
AND SOLD BY
J. STRATFORD, No. 112, HOLBORN-HILL.

1807.

PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, G. Cumberland, Esq.; X.; Mr. John Tatum, Jun.; Mr. John Webster; Mr. W. Skrimshire, Jun.; G. C.; John Bostock, M. D.; Mr. J. Hume; R. B.; Mr. Knox; T. Thomson, M. D. F. R. S. E.; Dr. Halliday; Sir H. C. Englefield, Bart. M. P. F. R. S.; David Brewster, M. A.; A.; A Constant Reader.

Of Foreign Works, Dr. Carradori; M. Montgolfier; M. Bouillon Lagrange; M. Ch. Hersart; Professor Proust; M. De Lalande; Samuel Mitchell.

And of British Memoirs abridged or extracted, Mr. John Gough; Mr. William Watson; Dr. J. A. Hamilton, Dean of Cloyne; Rev. James Little; Thomas Andrew Knight, Esq. F. R. S.; Matthew Flinders. Esq.; Mr. W. Hardy; Mr. Andrew Flint; James Smithson, Esq. F. R. S.; G. Mitchell, M. B.; Patrick Neill, A. M.; Mr. Peter Herbert; Mr. John Antis; J. C. Curwen, Esq. M. P.; John Pond, Esq.; Sir John Sinclair, Bart. M. P.; Dr. William Roxburgh; Mr. H. Steinhauer; Rev. Peter Roberts, A. M.; Dr. Cogan; Rev. William Richardson; Mr. George Gilpin; Mr. S. Grandi; Mr. Neill Snodgrass; Thomas Egan, M. D. F. R. S.; William Alexander, M. D.; John Alderson, M. D.; Rev. Gilbert Ausein, M. R I. A.; Mr. Edward Martin.

Of the Engravings the Subjects are, 1. A Diagram by Mr. John Gough, to illustrate the Doctrine of mixed Gases; 2. A very simple Scale for Drawing the Vanishing Lines in Perspective, by G. Cumberland, Esq.; 3. Figures by Dr. Herschell exhibiting the singular Outline of the Disk of Saturn; 4. Three Figures exhibiting the Acoustic Experiment of the Invisible Girl: 5. An expanding Band Wheel for driving Machinery, by Mr. Flint: 6. A new Door Latch, by Mr. Antis: 7. An improved Book Case Bolt, by Mr. Peter Herbert: 8 Mr. Hardy's Compensation Balance for a Time Keeper: 9. A Calorimeter for measuring the relative Effects of Fuel, by M. Montgolfier: 10. Apparatus used in the Manufactory of forged Iron Vessels: 11. An Astronomical Circle, by Mr. Troughton: 12. A Drag for raising the Bodies of drowning Persons, by Dr. Cogan: 13. Sir H. C. Englefield's Method of adjusting the Transit Instrument: 14. A new Astrometer, by David Brewster, A. M.;

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A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY AND THE ARTS.

JANUARY, 1807.

ARTICLE I.

Description of a very simple and useful Scale, for dividing the Vanishing Lines in Perspective. In a Letter from G. CUMBERLAND, *Esq.*

To Mr. NICHOLSON.

SIR,

Introduction.

A VARIETY of occupations and speculations have of late forced me to neglect some former engagements, to use my poor endeavours in promoting the laudable ends of your truly interesting publication.

But that I may not be thought to have entirely forgot them, I send you the following trifle, which, however simple the idea, will, I am sure, be the more valued by you on that very account, if I have justly estimated the noble simplicity of your intelligent mind.

The valuable art of Perspective.

Having been in the habit of drawing for my amusement all my life, and feeling the value of that acquirement, it has been my practice to recommend to others as much of that acquisition as can with very little trouble be attained; I mean the putting into perspective common objects; such as simple landscapes, machines, buildings, and the interior of apartments, manufactories, &c. And where I have had an opportunity to [illegible] four or [illegible] these lessons, I have generally seen my [illegible] [illegible] satisfaction, without ever shewing them [illegible] other voluminous treatise; books that have hindered [illegible] from the study of art, than they have ever made artists; for a moment's consideration on this subject will convince any mind, capable of reflection, that, to accomplish the general ends that even most painters have in view with respect to that art, it is only necessary to know the use of *the points of sight and horizontal line*. For while men have agreed to avoid bevel lines in all their constructions that are intended for use or habitation, we shall only want as much knowledge of the art as will enable us to put these into perspective, and to assit us at first, before, by practice, we have attained a correct eye; for practice, daily practice, will soon do all the rest, even by barely drawing the interior of a large apartment or gallery, with the objects continually before us in common use.

May be very easily acquired.

He who can put horizontal and perpendicular lines into perspective, will soon be able to [illegible] every thing [illegible].

Simple contrivance for Vanishing-Lines.

To save time, however, and to imprint the few lessons necessary to be given on the mind of a learner, I have, some time back, made use of the following simple contrivance; which I now send to you, as the most likely means of universally promoting this necessary preliminary study, where the first general principles have been instilled:—Take a sheet of paper of an octavo size, and rule it with very black ink, from A to B (Fig. 1, Plate I). This represents the horizontal line; then fix a point in the centre, at C; this we will call the moveable point of sight: afterwards cross it, as in the plate, with as many diagonal lines as you please; and thus you have an instrument prepared that will be a sure guide to an inexperienced eye, in taking the perspective lines of all objects placed at right angles; such as streets, buildings, churches, apartments, &c. by merely placing it under the leaf you mean to draw them on from nature, so as to see them faintly through, as boys do their writing-copies, when young and inexperienced.

It is a ruled paper, having the lines thereon.

A correspond-ing plate [illegible] glass having similar lines.

But, to make this instrument more complete, we should add a plate of glass of the same size as the leaf of the drawing-book; on which the like dark lines should be drawn so as, by holding it up perpendicularly, we may see, and, as it were, render tangible, the truth of perspective lines of buildings; and for those whose sight is bad, or for very young people, it would not be amiss to take a copper-plate of the like dimensions, and with a fine needle gently scratch out the like lines, in which

—or a copper-plate, or print-[illegible]print lines.

case there will be no necessity to take off the burrs, as the engravers call the ridges raised in ploughing copper; and, from this plate, *ten thousand* impressions may be taken of the faint lines, by way of guide, on the drawing-book of a young beginner, without injuring the plate; for I can assure your readers, that it is more difficult to erase a slight scratch from a sharp needle on copper, by the act of taking impressions, than the deepest cut of the graver; the reason of which is, that the ridges of the skin of the printer's hand can never enter that fine line, whereas, in a coarse one, he polishes the edges of it down by every operation, and thus renders it a smooth channel, at last undefined, and incapable of retaining the printing ink; and the reason I am so diffuse on the subject is, that I think the knowledge of it may be generally useful, particularly to those who wish to extend the publication of botanical outlines: as it is not necessary to be taught the art of engraving for those who can draw lines, to design on copper the peculiarities of plants, or their anatomy. How to trace deeper lines with certainty on copper as easily as on paper, I will have the pleasure to communicate to you at my next leisure moment.

Utility of engraving with the dry needle.

— particularly in botany.

But, to return to our subject.

To this simple contrivance, we may add a sheet of perpendicular lines, by which means the uprights will all be shewn; and for very heavy intellects, at first even the horizontal scale might be useful, though I never found it so among my acquaintance. There are also many little helps of simple contrivances to further the first acquirement of this plain branch of the art; that, if you approve the idea, I shall with pleasure transfer from my portfolio: but with respect to the application of this already described, it will be necessary to permise, that the scale should be longer than the drawing-book each way; by which means, by barely sliding it to the right or left, you can at pleasure place your point of sight more or less to the right, or left, or middle, of the horizon; and, to be prepared for all circumstances, it would be as well to be provided also with a scale having a high horizon, and another with a very low one, such as the Dutch painters generally used, and which ever produces a picturesque effect, by giving many profiles of the elevations, and multiplying the lines of light.

Sheet for perpendicular lines.

Thus you have an easy expedient for a first help—practice will accomplish the rest; for we all know, or should know,

This easy expedient and practice will complete the artist.

that daily practice discloses to the industrious draftsman all the arcana of optical, aereal, and linear perspective, destitute, it is true, of terms to describe his acquirement; but to his own mind a perfectly intelligible and useful rule, by the help of which he can, with certainty, imitate all he sees on the theatre of the universe.

With respect and esteem,
I am, Sir,
Your obliged humble Servant,
Bristol, Dec. 4. 1806. G. CUMBERLAND.

An Essay on the Theory of mixed Gases, and the State of Water in the Atmosphere. By Mr. JOHN GOUGH. *Communicated by Dr.* HOLME.

The Essays of Mr. Dalton adverted to.

FOUR essays appear in the fifth volume of the Memoirs of the Literary and Philosophical Society of Manchester, which contain many new ideas relating to the constitution of mixed gases, and the sate of water in the atmosphere. The design of these papers is evidently intended to remove certain difficulties which must strike every man of science, who happens to peruse M. de Luc's Theory of atmospherical Vapour. This attempt has the double recommendation of ingenuity and novelty; but the leading opinions of the system, even in its present form, are liable to several objections, which I am going to point out, being generously invited to undertake the task, by the author himself. My doubts relative to the subject arise partly from mathematical considerations, and in part from the evidence of experiment. Certain objections of the first class dispose me to conclude, that an atmosphere constructed on Mr. Dalton's plan, will appear upon examination to be repugnant to the principles of the mechanical phylosophy; and a direct appeal to experiment has moreover convinced me, that well established facts contradict the essential points of the theory.

Concise view of Mr. Dalton's Theory of mixed gases.

To begin with the objections of the former class: I am ready to admit the existence of a fluid mixture, such as we find described at page 543, in the* fifth volume of the Manchester

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* See our Journal, Vol. VI, p. 251.

Examination of Mr. Dalton's theory of mixed gases.

Memoirs, with this reservation, that the concession is made, merely for the purpose of shewing such a combination to be incompatible with the usal course of things, for a moment; which being demonstrated, the inutility of the fundamental hypothesis will follow, as a necessary consequence.—To give a concise view of Mr. Dalton's general notion of the subject, we are to suppose a number of distinct gases to be confined in a space common to them all; which space may be circumscribed by the concave surface of a vessel, or the compressing power of an external fluid: besides this, we must imagine the constituent particles of each individuel gas to be actuated by a mutual repulsion, while, at the same time, they remain perfectly indifferent to the particles which compose the other fluids that are confined in the common space; in short, we are to conceive, that the particles of each gas act upon those of their own kind in the manner of elastic bodies; but that they obey the laws of inelastic bodies, as often as they interfere with corpuscles of a different denomination. After premising the preceding particulars, we may conceive a certain arrangement of the elementary parts of a fluid mixture, in which the adjustement of the whole shall be of a description which will form, from particles of any one denomination, a homogeneous fluid, possessing its own separate equilibrium; consequently, each gas will exist as an independant being, and exercise the functions of its elasticity, just as if all the other fluids were withdrawn from the common space. This systematic arrangement in an assemblage of gaseous substances cannot be maintained, unless one particular method of disposing its component parts be observed; wich consists in that distribution of the elements which will produce a separate equilibrium in the fluid composed by the elementary corpuscles of each denomination, consequently, the equilibrium in question cannot take place unles the necessary disposition of the heterogeneous particles be first established; so that the former requisite of the theory is entirely depended on the latter.—After having acquired a distinct idea of a fluid mixture, composed of gases possessing separate equilibria, we come in the next place to investigate the mechanical properties of such a compound; in the prosecution of which enquiry, the comparative densities of the constituent fluids must be first determined in a horizontal plane, the situation of which is given in the common space.

Examination of Mr. Dalton's theory of mixed gases.

Let the figure P M I N K V, Plate I, Fig. 2, represent this space, in which M V N K is the given plane.—Now since every point of this plane may be supposed to be at an equal distance from the earth's centre, the density of every homogeneous gas supported by it, will be the same in all parts of it. Let the constituent fluids be denominated A and B; also let C denote the compound; moreover let the densities of A and B, at P, be p and q; let P X and X Y be two equal evanescent parts of the line P V. Now seeing the pressure acting upon an elastic fluid is as the density of it, the fluxionarie increments of p and q, are as these quantities; but the densities of A and B, in the point X, are equal to the sums of p and q united to their increments respectively; let these sums be called e and f; then e is to f as p is to q, by composition of proportion: in like manner we find the density of A at Y to be to that of B at the same point as e is to f; i. e. as p is to q; thence it follows that the fluxionary increments of the two densities have universally the givin ratio of p to q; consequently the contemporary fluents, or the densities themselves have the same given ratio; now what has been proved of the two gases A and B may be extended to any other number; viz. the ratios of their densities, on the same horizontal plane will be given.

The ratio of A B, &c. being found to be constant, we can proceed to investigate the proportions of the quantities of matter contained in these fluids. Let D and d be the densities of A and B, in the plane MKNV; also let W and w be the quantities of matter of each kind, contained in the variable space PMKNV; call PV x, and the area of the plane MKNV y: now the fluxion of the space PMKNV is exprssed by y into the fluxion of x; moreover, the quantities of matter in two solids are in the complicate ratios of their magnitudes and densities, or in that of their densities only, if their magnitudes be equal; therefore the fluxion of W is to that of w as D is to d; because the fluxionary magnitude is common both to W and w; but D is to d as p to q, a constant ratio; consequently fluxion of W is to fluxion of w as p is to q; therefore W has to w the same given ratio; that is, the matter in A is to the matter in B as p is to q. In the next place, let R and r be the distances of the centres of gravity of A and B, from the point P, taken in the line PI: then R into the fluxion of W is equal to the product of D, Y, x, and the fluxion of x, from a well

Examination of Mr. Dalton's theory of mixed gases.

known theorem in mechanics; for the same reason, r into the fluxion of w is equal to the product of d, y, x and fluxion x; hence R into fluxion of W is to r into fluxion of w, as D is to d; but D is to d, as fluxion W is to fluxion w; therefore R and r are equal: consequently the centres of gravity of A and B coincide, and the point of their coincidence is also the centre of the sytem C. Thus it appears, that when the component gases of a fluid mixture possess separate equilibria, their densities are every where in a given ratio; and they have a common centre of gravity; the converse of which is equally true, viz. if their densities be not every where in a given ratio, and if they have not a common centre of gravity, they do not possess separate equilibria.

It is necessary to observe, in this stage of the inquiry, that though we admit the particles of A and B to be inelastic in relation to each other, the concession must be strictly confined to the particles themselves; for the gases which are composed of them are elastic bodies: they therefore receive and communicate motion according to the laws which are peculiar to bodies of this description. The foregoing properties of a fluid mixture, which has been supposed to be duly adjusted, is now to be used in the examination of the fundamental proposition of the new theory intended to explain the constitution of the atmosphere. According to this proposition, if two gases come into contact, the particles of which are perfectly inelastic in respect of each other, the particles of A meeting with no repulsion from those of B, further than that repulsion, which, as obstacles in the way they may exert, would instantly recede from each other as far as possible in their circumstances, and consequently arrange themselves just as in a void space. The preceding are the words of the author of the Theory; and it is readily granted that the particles of such a heterogeneous mixture would recede from each other as far as circumstances will permit; the present subject of inquiry then brings the dispute to this issue—can that arrangement take place amongst the particles of two or more gases, which will make their centres of gravity coincide in one point? For the separate equilibria of the fluids, which enter into the constitution of the compound, will not be established until this arrangement be perfectly formed. The completion of this process being essential to the new theory, the effect of it has been, perhaps, too

Examination of Mr. Dalton's theory of mixed gases.

hastily inferred in the fourth proposition of Mr. Dalton's first essay; for I am sorry to observe, that the inference is not supported by demonstration, drawn from the doctrine of mechanics. It is the business of the present essay to supply what has been omitted, and to investigate the consequences which must arise from the collision of two heterogeneous gases, differing in their specific gravities.

The existence of the fluid mixture, required by the theory, has been granted already, for the sake of argument; and in order to continue the inquiry, it must be remarked at present, that the necessary internal arrangement of the compound C, is liable to be disturbed perpetually by accidents resulting from the course of things; to which course the author of the theory undoubtedly wishes to accommodate his ideas. The preceding assertion may be exemplified in a manner which is familiar, and may be applied with ease to natural phenomena: let us suppose then an additional quantity of the gas A to be thrown into the pneumatic apparatus, containing the compound C, which was in a state of proper adjustment previous to this event. No one will imagine, that this fresh matter can diffuse itself through the mass of C with the same expedition that the electric fluid shews in expanding along a conductor: this supposition is contradicted by various appearances, from which the following one is selected; agitation is known to accelerate the union of oxigen and nitrous gas. The quantity of A then, which has been newly admitted, will remain at first unmixed with B; but it will act immediately with a repulsive force upon kindred particles diffused through the compound C. This new modification of A will not preserve the density of its parts every where in a constant ratio, to the density of the corresponding parts of B; and this change will disjoin the centres of gravity of A and B; which has been proved above. But when these points are placed apart, the separate equilibria of the fluids cease to exist, which has also been demonstrated before; therefore A and B begin to act and react mutually; which circumstance disturbs the necessary adjustment of C, and forces it to assume another character. It has also been proved in a former paragraph, that the two fluids will act upon each other in the manner of elastic bodies, even when the heterogeneous particles are supposed to be mutually inelastic; consequently A and B will begin to obey the

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law of their specific gravities, as soon as their centres of gravity are separated by introducing into the space occupied by C, a fresh quantity of A or B: in consequence of this alteration the centre of gravity of the heavier fluid will begin to descend while that of the lighter moves upwards. When once the centres of two gases are placed apart, their separation will become permanent; because, when at a distance, they are urged in opposite directions by a force resulting from the difference of the specific weights of the two fluids; and this contrariety of efforts must continue so long as the two centres are disjoined; consequently this opposition of force must be lasting; seeing nothing can put an end to it but an union, which it will always prevent. Nor can the mutual repulsion of the constituent particles of each gas, considered apart, in any manner promote the junction of the centres of gravity of the two fluids; because the action and reaction of a number of bodies amongst themselves do not alter the state of their common centre of gravity, whether it be at rest or in motion: so that A and B are under the necessity of observing the law of their specific gravities, just as if the kindred particles of each fluid were actuated by no reciprocal repulsion nor any other cause of reaction. The doctrine of gases, which are mutually inelastic, is rendered indefensible by the preceding arguments; for the hypothesis is thereby exposed to a difficulty which the author of the theory justly remarks, makes a mixture of mutually repulsive gases of different specific gravities an improbable conjecture; so that his own objection ultimately discountenances the leading opinions of that theory which it induced him to adopt in particular. At the same time, philosophers are convinced that the atmosphere is a compound of gases, possessing various degrees of specific weight: they moreover know, that different chemical agents perpetually disturb the equilibrium of the compound, as some of them constantly absorb while others unfold the gases of which it is composed. The preceding facts are certain: consequently the heterogeneous elements of the atmosphere must be united by a common tie, which may be denominated a species of affinity, at least while our knowledge of the subject remains in its present imperfect state. The transparency of the great body of air surrounding the earth, also affords a strong argument for the chemical union of its component fluids; and, at the same

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time, discountenances the idea of the compound being a mechanical mixture of any description whatever; for when a number of diaphanous bodies of different specific gravities are mixed together, they form an aggregate which is opaque; but the union of the substances by fusion renders the mass transparent in many instances. Now as the atmosphere is diaphanous, we are obliged, by the principles of sound argument, to consider it in the light of a compound, the ingredients of which are united by a chemical tie.—Whatever may be the condition of the elastic fluids which enter into the composition of common air, one thing is certain from a preceding paragraph of this Essay; namely, no one of them can maintain a separate equilibrium as long as it makes an individual of the aggregate; consequently, each particle of the compound must be urged by a force resulting from the general action of the mass, not by a pressure occasioned by a particular member of it.

On this account, it is impossible for the acqueous part of common air to preserve the character of a gas at low temperatures; because steam cannot support 30 inches of mercury unless it is heated to 212 degrees of Farenheit's thermometer; were it then practicable to mix vapour of a less heat with atmospherical air, the spring of the gases would reduce it in an instant to the state of a liquid; so that the difficulty, which renders De Luc's theory objectionable in its original form, is not removed in reality by the present modification of it.

The theory of mixed gases has been found to be indefensible on the principles of the mechanical philosophy; and I suspect that part of it which relates to the separate existence of vapour in the atmosphere, will prove equally unfortunate when brought to the test of experiment. Mr. Dalton, in all probability, supposed he had done all that the confirmation of this theory required, by inventing the doctrine of separate equilibria, for nothing more has been offered in support of his opinions, particularly of that relating to the existence of uncombined vapour pervading the atmosphere, unless the statement of the following experiment, with his explanation of it, may be referred to this head. If two parcels of dry air, which are equal in bulk, density and temperature, be confined by equal columns of mercury, in two tubes of equal bores, one of which is wet and the other dry; the air, which is thus ex-

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posed to water, will expand more than that which is kept dry, provided the corresponding augmentations of their temperatures be equal; which phenomenon is thus explained on the principles of the theory. The vapour that arises from the sides of the wet tube, possesses a spring of its own; therefore it takes off part of the weight of the mercury from the air, and thereby leaves it to expand itself, so as to re-adjust the equilibrium. According to this explanation, if l and g represent the length of the columns of dry and moist air at any temperature; and if c denote the length of a column of mercury, equal in weight to the pressure that confines the contents of the tubes; and if f be put for the spring of vapour of the same temperature measured by a column of mercury, we have

$$g = \frac{lc}{c-f}; \quad \text{from which we also get } c = \frac{fg}{g-l}$$

the last expression affords us an opportunity of comparing the preceding explanation, and therefore the theory itself with facts; for, according to the experiments of Mr. Schmidt, 1000 parts of dry air at 32 degrees of Farenheit, will expand [illegible] 1087,11 parts, by being raised to 59 degrees, in contact with water; call this number g: according to the same author, 1000 parts of dry air at 32 degrees will expand to 1053,61 parts, by being heated to 59 degrees in a dry tube; let this number be l; then $g-l = 33.50$: but f, or the spring of vapour at 59 degrees, is .507, according to Mr. Dalton; then $fg = 551, 164$; hence $c = 16,15$ inches; which expresses the height of the barometer, together with the column of mercury contained in the tube. If the temperature be stated at 95 degrees, c will amount to little more than 8 inches: now it is highly improbable that Mr. Schmidt made his experiments when the barometer stood at a height indicated by either of these numbers.—This application of the theory to practice, affords a presumptive evidence that the principles of it are not altogether just, supposing the experiments of Mr. Dalton and Mr. Schmidt to be correct: but a positive proof of a want of accuracy in these principles may be obtained by introducing a small change into the manner of conducting the experiment made with moist air. This alteration consists in discarding the stopple of mercury, and substituting the simple pressure of the

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atmosphere in the room of it: because when this substance, which is impenetrable to steam, has been removed, the redundant vapour will, according to the theory, flow into the atmosphere, thereby leaving the moist air of the tube to follow the law of expansion observed by dry air. With a view to find whether this be the case or not, I filled a bottle with running water of the temperature of 59 degrees, which, when carefully poured out again, weighed 7794 grains. The bottle, having a dew left sticking to the sides of it, was placed in water at the temperature of 126 degrees: the mouth, which remained about an inch above the surface, was covered with my hand, care being taken to remove it frequently for an instant to permit the vapour and expanding air to escape. After keeping it in this situation about two minutes, I secured the mouth in the manner described above, and inverted it in a quantity of the same water, where it was reduced to 59 degrees; in consequence of which it took up 1622 grains of water, leaving a space equivalent to 6172 grains. If the experiment be now inverted, 6172 parts of air will occupy the space of 7794 such parts when its temperature is raised from 59 to 126 degrees; which is nearly double the expansion of dry air in like circumstances. For, according to Mr. Schmidt's experiments, 1000 parts of dry air of 59 degrees will become equal to 1133,03 such parts, by being heated to 126 degrees; therefore, by the rule of proportion, if 1000 parts give an expansion of 1133,03 such parts, 6172 parts give only 820: but the difference of 7794 and 6172 is 1622, which is nearly the double of 820. The preceding experiment, and others which I have made of the same kind, demonstrate that moist air expands more than dry air under like circumstances; and the fact subverts the notion of uncombined elastic vapour mixing with the atmosphere. The accuracy of the fact may be disputed; the doubt however is removed by repeating the experiment: but so long as my statement remains uncontradicted, the consequences of it to the theory in question, cannot be controverted by argument: for if elastic vapour mix with the air, it does more than merely enter the pores of this fluid: for, according to my experiment, it enlarges these pores at low temperatures, which we know to be impossible, unless the heat of the compound arises to 212 degrees. Those who are convinced of the superior expansion of moist air, will readily apply the principle to certain interesting phenomena, in particular to the origin of Tornadoes in hot countries, and the variation of the barometer in temperate climates.

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Mr. Barrow, an intelligent traveller in South Africa, observes, that the atmosphere in Caffraria is sometimes heated to 102 or 104 degrees: this is succeeded by local thunder storm, attended with heavy falls of rain and hail, as well as violent hurricanes. I do not pretend to assign the refrigerating cause, or the agent that produces precipitation in this case; I only have to observe, that the portion of air must lose much of its elasticity, which is suddenly cooled to 70 or 72 degrees, and at the same time parts with the water it held in solution. This partial diminution of spring will destroy the equilibrium of the adjacent parts of the atmosphere, and may be supposed to produce the tornadoes of the tropical regions. The same cause probably gives rise to the fluctuations of the barometer in milder climates; for though the changes of temperature are less in the milder than in the hottest parts of the globe, the agents that precipitate the water of the atmosphere, appear to act on a more extensive scale, and through a longer duration in the former situations than they do in the latter. Wet weather is neither momentary nor local in Europe; provinces, and even kingdoms are deluged with rain for weeks together. The air, which discharges such an abundance of water, will lose part of its spring, according to Mr. Schmidt's experiments, even when it suffers no change of temperature: now it is evident that the equilibrium cannot be restored in an instant; because the diminished elasticity must be augmented in this case by currents of air coming from remote places. The diminution of spring in the atmosphere is shewn by the fall of the barometer; and the subsequent ascent of the mercury indicates the arrival of the restorative currents. According to this explanation, the barometer will rise slowly but gradually in the centre of the rainy district, while the motions of it will be more rapid and less regular towards the verge of the storm. High winds will also prevail in wet seasons, which will blow towards the parts where the elastic force of the air is least; that is, where the rains are most abundant.—I know not what claim to originality is due to the foregoing hints towards the theory of the barometer; they have, however, the merit of being a natural consequence of an established fact; I mean the great dilatation of air saturated with moisture, which must undergo a proportionate contraction when deprived of water.

On the comparative Culture of Turnips. By Mr. William Watson*.

Culture of Turneps.

HAVING been long, and pretty extensively employed in Agriculture, in a district where the turnip husbandry is much practised, and being satisfied that when the soil is proper, and the management judicious, great crops of that invaluable root are the most profitable means of obtaining luxuriant and productive crops of corn, &c. and of laying a solid foundation for future abundance in the increasing quantity of manure, I have paid particular attention to the different modes pursued in its cultivation. It is with great pleasure, therefore, that in the list of premiums offered by the Society, instituted at London, for the Encouragement of Art, &c.—a Society whose patriotic and laudable exertions deserve the most warm and grateful thanks of every real friend to the British empire,—I observe one for the best set of experiments made with a view of ascertaining the most advantageous of these modes; and, having made a comparative trial with great accuracy, I beg leave to request that you will do me the honour of laying this paper, which contains an account of it, before the Society. That there are situations in this kingdom in which eight acres of land may be found of an *uniform* quality, I do not doubt. I must, however, remark, that I never found that number of acres contiguous to each other, or properly situated, for an accurate comparative experiment, in the fallow land of any farm in which I have been concerned, so *precisely similar in soil and condition*, as to induce me to think that I could have exhibited the result of so extensive an experiment as irrefragable evidence of the superiority of any particular mode of culture. Besides I could not have attended either to the minute mixing of the necessary quantity of dung for eight acres of ground, *so as to have rendered it of an uniform quality*, nor to the weighing of *all* the turnips upon that quantity of land, without which, (when I adverted to the difference of weight occasioned even by a scarcely perceptible difference in the diameters of similiar solids) I could not have totally divested myself of some doubts as to the accuracy of the result.

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* Society of Arts, Vol. XXII.

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To these reasons, I could not *satisfactorily* conduct the experiment on so large a scale as that proposed by the Society; and though I am thereby prevented from becoming a candidate for the Medal,—a reward by which I should have considered myself highly honoured,—yet I hope this Communication will not be deemed altogether unimportant; and that it will, in some degree, forward the views of so distinguished a body.

Every part of the ground upon which this experiment was made, had been managed for a series of years, in exactly the same manner. After being three years in grass, it produced a crop of oats in 1802; in the autum of which year it was once ploughed. In May and June following, it received three furrows in the common way, and was completely pulverized and cleaned; after which it was divided into four flat ridges, about eight yards broad, each ridge containing precisely 4719 square feet. The soil is a dry, light sandy loam, mixed with small hard stones, incumbent on a thick sudstratum of gravel; and the four ridges were so much alike soil and condition, that I think I may assert, that the most accurate chemical operator could not have proved the smallest difference in these respects. On the 22d of June last, the ridge, No. 1, was manured with dung; immediately after which, the manure was regularly spread over it, and ploughed in. The whole ridge then received a single working, with a light short-tired harrow; and *while the moisture was fresh*, the turnip-seed was sown with a machine, in rows, upon a flat surface with thirteen inches intervals. About the same hour, the ridge, No. 2, was prepared and formed into small ridges, or drills, upon which the turnip-seed was deposited in rows, with a machine, twenty-six inches from each other. *The dung in about one third of the raised drills on this ridge was partly left without being completely covered in.*

Early the next morning, the ridge, No. 3, was also formed into small ridges, or drills, with intervals of twenty-six inches. On the tops of these ridges, a proper machine quickly deposited the turnip-seed in single rows, precisely in the same mode as that pursued in No. 2. On this ridge, however, No.

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3, *every atom* of the dung was carefully covered with the plough. Immediately after No. 3 was finished, No. 4 was dunged and sown with turnip-seed, in the usual manner, in the broad-cast method.—Every part of the four ridges was manured with dung of the same quality. It was not *thoroughly* rotten, but had arrived at a more advanced stage of putrifaction than that used by farmers in general; and, in order that its quality might be uniform, it was carefully taken from *one* part of the fold-yard, and well turned over, and mixed in the field*. An equal quantity was applied to each ridge, at the rate of fifteen two horse cart-loads † per acre. The turnip-seed was likewise of the same quality and kind, and was sown on each ridge at the rate of about one pound and a half per acre, The succeeding weather was remarkably dry and unfavourable for the growth of the turnips, only one light shower having fallen, from the time the seed was committed to the ground, to the 16th of September following.—Notwithstanding this, however, the whole of the four ridges planted exceedingly well, though not so early as I could have wished; and their progress into the rough leaf, as well as their appearance for some time afterwards, was propitious. From the extreme severity of the drought, however, and the natural dryness of the land many of the plants in every ridge were killed. No. 1 lost the greatest quantity; No. 2 the next, *especially on those drills where the dung was not all completely covered in*; and No. 4 scarcely so many as No. 3.—Throughout the whole crop, vegetation seemed extremely languid, and the turnips were generally of a small size; the largest were produced on Nos. 2 and 3, *in the drills with intervals of 26 inches*. These intervals were twice horsehoe. In these rows the plants were left about eleven inches asunder. Numbers 1 and 4, in which the plants were set out at about twelve inches from each other, were thrice hand-hoed with great accuracy. The several operations of plouhing, sowing, and hoeing, were performed in the same kind of weather on each ridge. I attended the whole of them myself, and can safely say that the utmost precision and impartia-

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* Dung was the only manure applied.

† The cart was five feet three inches long, three feet three inches broad, and one foot six inches high, in the inside.

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lity were observed. The four ridges were carefully surrounded with proper rails to prevent damage, and no depredations of any kind were committed *. On the first of this month, *all* the turneps which were produced on these ridges were drawn up, and carefully and exactly weighed, after their tops and tap, or fibrous roots, had been cut off. The produce of each ridge was as under:—

	stones.	*lbs.*	*lbs.*
No. 1, drilled on a flat surface, with intervals of 13 inches	144	10	--14 to the stone.
No. 2, drilled on small ridges, with intervals of 26 inches, and with a part of the dung not perfectly covered in	193	5	—ditto.
No. 3, drilled on small ridges, with intervals of 26 inches, and all the dung well covered in	211	4	—ditto.
No. 4, broad cast . . .	168	12	—ditto.

Remarks on the different Modes of Culture.

No. I.

In this method of management the dung is applied in a manner exactly similar to that practised in the broad-cast husbandry; and experienced agriculturists well know, that *even after it has been thoroughly putrefied*, it cannot be *wholly* covered by the earth in the mode of ploughing, pursued under that system of cultivation. In almost all cases, the harrows are used to produce an even surface after the last ploughing, and immediately before the seed is committed to the ground. By this operation more of the dung is left upon the surface; and when it is considered that much of it is applied in a long or half-rotten state, it will readily be conceived, that a still greater quantity will be left exposed on the surface of the ground; in which situation it can conduce but little, if any thing, to increase its fertility.

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*. Except that a mole destroyed a few plants on three drills on No. 1.

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Under this mode of management, the plants may be left at more regular distances in hoeing than in the broad-cast method; but I am *now* inclined to dispute that that operation can be performed at an expence materially, if at all, less than among those obtained in the latter way. The plants are generally left in the rows at about twelve inches apart, so that an acre will produce about 40,200 turneps, when the crop is a full one.

No. II. and III.

Some practical agriculturists, as well as chemical philosophers, have contended, that dung should be *thoroughly* putrefied before it be applied to the soil; and others maintain, that it is more beneficial to apply it in a half-rotten state. Into this dispute, I am not, at present, inclined to enter. Let it suffice to say, that a great majority, probably upwards of three-fourths of the farmers, in almost all the extensive turnip districts in the kingdom, apply it either in the latter state, or before it has arrived at a much more forward stage of putrefaction; and if rotten dung (thoroughly putrefied) cannot be *wholly* covered in this common mode of ploughing, it is obvious, as I have before remarked, that, in the other state, a still greater part must be rendered nearly useless by exposure to the solar rays, &c. In the management now under consideration, however, every atom of it may be buried, if the spreaders and ploughmen are attentive. That management is as follows: As soon as the land has been properly pulverized and cleaned, a double-mould board plough, drawn by two horses, is used to raise small ridges, about 12 or 14 inches high, with intervals of twenty-six inches, and the tops, of about an inch or two broad. All the drills should be equal in size. The height should in some measure be regulated by the quantity and state of the dung. Immediately after the small ridges or drills are formed, a man with a cart, drawn by one or two horses, lays a sufficient quantity of dung for three or five drills (in small heaps), in the interval, while the wheels of the cart run in the adjoining spaces. In this manner all the other intervals are manured. As soon as the dung is carefully spread in the bottoms of the intervals, another double-mould board plough (also drawn by two horses moving in the intervals), splits the ridges along their tops. This operation

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completely covers the *whole* of the dung, and reverses the tops and intervals. A roller about ten inches diameter, and four feet in length, drawn by one horse, is now moved along the ridges. It covers two at a time, leaving the tops generally about ten or twelve inches broad, in the middle of which the turnep-seed is deposited, in a rut made by the coulter of the sowing machine, which is fastened to the hinder part of the above roller by a cord about nine feet long; the distance between each row of turnip-seed, being twenty-six inches; and if the ploughing and spreading have been properly performed, the dung will be nearly beneath the rows. Thus the agriculturist is not subject to the waste of any part of his manure, and reaps the superior benefit of having the turnep-seed regularly sown, in a rut of a proper depth, penetrating nearly to the dung in the middle of the small ridges;—a method which seems better calculated to give to the cultivator of the field advantages similar to the rapid and vigorous vegetation promoted by the *hot-bed* of the garden, than perhaps any other mode of culture. The importance of having *all* the dung perfectly covered, is evinced by the result of the above experiment; for, with the exception of a small part of it, in a few drills on No. II., not being perfectly covered with the soil, there was no difference *whatever* between the management of that ridge and the mode pursued on No. III. In dry weather, the roller is moved twice along each ridge, first to compress the soil, and next to close the rut made by the coulter of the sowing machine, to secure the turnep-seed from depredation and drought: but if the soil be so moist as to stick to the roller, it is moved only once along each drill; and some able husbandmen are of opinion, that this is the most advantageous mode *in any state of the soil;* that without the second rolling, the turnip-seed will vegetate regularly; and that, while young and tender, the plants will be beneficially sheltered by the rut of the sowing-machine in adverse weather. Some cultivators form the drills, or small ridges, with a common single plough, and in many situations they are made more straight and neat than with the double plough. With the latter, however, they may, in most situations, be sufficiently well formed, at about half of the expence incurred by using the single plough, *which does not cover the dung better than the other.*—The skuffler, an implement with three or five

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hoes is sometimes used to clean the intervals. Some, however, prefer using two small ploughs of the common form, four or five inches broad at the bottom, and fastened together by screws, which increase or diminish their distance from each other, according to the breadth of the intervals. This implement is drawn by one horse, and, by being moved *once* along each interval, cuts a proper quantity of earth from each side of the row of plants; and by proceeding in this manner, a ridge of earth is laid up in the middle of each interval. This mode is the best in situations where the drills are not perfectly straight. Where they are *quite* straight, an implement is used, which, instead of moving the earth from each side of one drill, cuts it off the inner sides of two drills; and in either method the hoeing of the intervals may be performed with equal expedition. A few weeks after these small ridges are formed in the middle of the intervals, they are generally split by a double plough drawn by one horse, the earth being laid close against the turneps on each side. These operations not only destroy the weeds in the intervals, but give to that part of the land the advantages of a bare fallowing, and, besides being *greatly* cheaper, are much more fertilizing than hand-hoeing. In this mode of cultivation the turnips attain a greater size than under the broad-cast method, or that with narrow intervals; and though the plants are generally left at about eleven inches apart in the rows, which reduces the number on an acre, when the crop is a full one, to about 21,900, the result of the above experiment will not be surprising, when it is considered, that from the properties of similar solids, the weights of well-formed (spherical) turneps are in the ratio of the cubes of their diameters, and consequently that one of eight inches and a half diameter will weigh nearly as much as three of six inches diameter each.—Nearly all the farmers in this district use their utmost endeavours to obtain turnips of a larger size, which, together with the other important advantages derived from it, has long induced them to prefer drilling on small ridges, with broad intervals, to any other mode of culture; and within the last twenty years, it has become the almost universal practice in the counties of Northumberland, Roxburgh, Berwick, and East Lothian,—an extensive and extremely well managed district; in which, I believe, the rents of land are considerably higher than in any

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other in this kingdom. In several, the drills are not drawn at right angles to the ridges (I mean the common ridges of the field), but in a diagonal direction; it having been found, that the seed-furrow in the succeeding spring, together with the effects of common harrowing, not only reduces the land to an *even* surface, but that after such management, the crops of corn are *uniformly* luxuriant and productive, the manured parts being, in these operations, well mixed with the soil in the intervals. I am satisfied, from my own practice, and pretty accurate observation on that of others, that with *considerably less* manure, as weighty a crop of turneps may be obtained by this method of cultivation, as by that with narrow intervals, or in the broad-cast husbandry; and, as it is generally difficult to raise as much dung as will manure the whole of the fallow land, at the rate of fourteen to sixteen loads an acre, this, *in promoting the growth of more extensively luxuriant crops, and increasing the quantity of manure for those which succeed,* is an invaluable advantage. Besides, in unpropitious seasons, when, under the broad-cast and narrow drill system, a judicious agriculturist would not cultivate turnips on land he has not been enabled *thoroughly* to pulverize and clean, he would venture to raise them where the spaces between the rows are sufficiently broad for the admission of the horse and the plough, under an idea that before their tops covered the intervals, (which they generally do about the beginning of October) his ground could be brought into a proper state.—You will no doubt remark, that the crop I obtained even on No. III., was but scanty; and conceive, however, notwithstanding that circumstance, that the experiment satisfactorily shews the superiority of the mode of management pursued on that ridge.—By the same mode, I obtained a crop on the land surrounding that on which the experiment was made, which, considering the extreme dryness of the summer, and that it was sown at the same late period of the season as that upon the experiment ground, may be reckoned a very productive one; and, as the soil was not superior in quality, it may be of some consequence to endeavour to account for this difference. The land marked out for the experiment, contained some couch and other weeds, which I wished to eradicate; it therefore received a common ploughing *only a few days previous to the seed being committed to the ground.* The

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surrounding land had lain for a much longer time between the last ploughing and the seed-furrow, and contained more moisture at the time of sowing them than the other; and though this, in a humid season, would not have caused a material difference in the crops; yet, in a summer so extremely dry as the last, it was attended with important advantages. To these I may add others; for dung having last year been unusually plentiful, it was manured with about *twenty* loads an acre, and with dung in a very *moist* state; whereas, that applied to the land on which the experiment was made, lost a considerable portion of its moisture by evaporation, during the time of mixing *well*, for the purpose of rendering *all* parts of it equal in quality.—Perhaps it may not be deemed unimportant to state, that the prevailing opinion is, that *very dry seasons are more unfavorable to the turnips raised on the small ridges (drills) than to those produced on land with a flat surface.*

No. IV.

The same objections which have been urged against the manner of applying on No. I. may be advanced against the mode of cultivation pursued on this ridge, under which the plants cannot be left with such precision and regularity as in the drill husbandry.

Expence of each mode of Culture.

The management pursued on Nos. I. and IV., is less expensive *up to the time the plants become fit for hoeing*, than that pursued on Nos. II. and III. This saving of expence, however, is overbalanced by the cheapness of hoeing under the latter mode, and by the advantages derived from that operation being performed before the plants become too large. The general expence of hoeing broad-cast turneps, in this quarter, is about seven to ten shillings per acre, of 4840 square yards. Those in drills, with narrow intervals, will cost as much; and when it is considered, that an acre of these contains twice as many rows as the same quantity of ground under the broad intervals, and that these intervals are quickly and efficaciously hoed with the horse and plough, it will be readily conceived that the latter mode is the least expensive *upon the whole*. As the turneps under this experiment did not grow uniformly, some parts were much sooner fit for hoeing than others. The person that hoed them was sometimes not employed among them above an hour in the day; which prevents my furnishing an accurate account of the expense of hoeing each ridge.

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So easy is the operation of hand-hoeing the small ridges or drills with broad intervals, that in this quarter, it is nearly all performed by women, boys, and girls. If we depended on men, as the farmers do in some other districts, we could not *perfectly* hoe much more than one third of our turnip crops.

I am, Sir,
Your most obedient Servant,
W. WATSON.

North Middleton, near Wooler,
by Belford, Northumberland,
Feb. 18*th*, 1804.

On Comparative Micrometer Measures. In a Letter from the Rev. Dr. J. A. HAMILTON, *Dean of Cloyne, to the Rev.* J. BRINKLEY, *F. R. S.*[*]

Observatory, Armach, Jan. 10, 1806.

DEAR SIR,

I BEG leave, through you, to communicate to our Academy the following paper, on comparative observations made with different kinds of micrometers; which, I hope, may be deemed worthy their notice. It was suggested to me, so long since as in the year 1794, that a comparative view of the result of the measures, made under similar circumstances, of the diameters of the heavenly bodies, with the different kinds of micrometers, that are now most generally used by astronomers, might have considerable use; as well in confirming the determinations of the values of the diameters, as given by former observations, as in deciding on the merits of the different instruments, and

Three methods of measuring small angles—the wire micrometer; divided object-glass; and the sextant.

[*] Irish Transactions, vol. X.

shewing, at one view, a sort of harmony of micrometers. *My own opinion, on this subject, entirely coinciding with that of the learned friend, who made this proposal,* I set about making comparative observations of the measures of the sun's diameters, as taken with the old wire micrometer, made in the best manner, by Mr. Dolland; with his divided object glass micrometer; and with a ten-inch reflecting sextant, executed, in a very capital style indeed, by Messrs. Troughton.

Description and account of the wire micrometer.

Its scale determined.

Before I proceed to the detail of the observations, it may be proper to premise a short account of the nature and adjustments of the several instruments, that were the subjects of this experiment. The wire micrometer, as its name denotes, measures intervals, by the separation of two moveable wires; these wires should perfectly coincide, when the index of the scale marks 0 or zero: and the quantity of the separation of the wires, made by the turning of the screw which effects it, is denoted by revolutions, and parts of revolutions, of the index, over a graduated circle, attached to the micrometer-screw; which, in this instrument, consists of fifty sub-divisions. There are several ways of ascertaining the values of these revolutions and sub-divisions, in arcs of a great circle in the heavens. The method which I adopted was this: the microscope being fitted to an achromatic telescope, on an equatorial stand, I carefully separated the wires by fifteen exact revolutions; and then turning round the whole system, till a fixed wire, at right angles to the measuring wires, was in a plane parallel to the equator, I measured, by the sydereal clock, the time the sun's limb, and various fixed stars took, to run along the fixed wire, from centre to centre of the measuring wires. This trial was very frequently and repeatedly made; and the stars and sun's limb, being all reduced to the equator, the general result gave 121″,1, for the equatorial interval of the fifteen revolutions. This interval, reduced to space, made each revolution of the figured head = to 2′ and 1″ of measure; and, of course, each of the fifty sub-divisions = to 2″,42 nearly of an arch of the equator. In making the subsequent measures of the sun's diameter, or that of any other celestial arc, the measure was always finished, by moving the wire in the direction in which the fifteen revolutions were originally made. The advantage of this micrometer is principally this: that, in adjusting the telescope, and the micrometer wires, to distinct vision, no alteration is made, by the difference of the conformation of the eye, or of focal distance, that suits that of the observer, in the value of the arc to be measured. The principal defects of it are: the difficulty of judging accurately of bisections, or contacts of the fine wires, by the limbs to be measured; and the impossibilt y of observing any diameter, except the one perpendicular to the equator.

Its scale determined.

The object-glass micrometer is an instrument, now so familiar to every person conversant in the use of astronomical instruments, that it is only necessary to say, that mine was made, and adapted to a triple object-glass achromatic telescope, of 42 inches focal distance, by Mr. Dolland, and its scale very carefully verified by himself; and that the scale is, as usual, divided into inches, 10ths, 20ths, and vernier divisions: that, when it is applied, it lengthens the focal ditance of the telescope about 6 inches: thus making it 48 inches, or 4 feet focal distance.

Dolland's object glass micrometer.

The advantages of this species of micrometer are: the large scale, the fine images formed, and the facility of measuring diameters in every possible direction. Its imperfections are: that, to different eyes, and under different circumstances of the same eye, the length of the focal distance, that suits distinct vision, will vary; and, of course, the quantity of the measures, given by the scale, are liable to a small variation. The goodness of the telescope is, also, in some degree, impaired, by the application of this contrivance of a divided object-glass.

Its advantages.

It should be noted, that the wire and object-glass micrometers, were both adapted, in their turns, to the same achromatic telescope; and the comparative observations made as near to each other, in point of time, as possible.

The diameters of the sun, measured by the ten-inch sextant, were taken with a small achromatic telescope, magnifying about twelve times, and were observed on the limb, and on the arch of excess, several times alternately; the measures being always finished in the same direction of the micrometer-screw: and the quarter of the double measure was used as the semidiameter, with the addition of 3″; which is the known diminution of the image of the sun's semidiameter, after the reflec-

Ten-inch sextant of Troughton.

tions and refractions it undergoes in the process. As the three kinds of micrometers, just described, are so completely different from each other, in their construction, adjustment, and mode of mensuration, I consider them as fully sufficient to make an experiment on the probable consistency of the results which may be obtained from different good micrometers; and shall now proceed to give a detail of the actual observations.

Observations of the sun's diameter.

1794.	S. Dr. ☉.	Semidiameters of ☉, as given in the Nautical Almanack.
August 26, D. O. G. Micr.	15′. 53″,835	15′. 53″,52.
Wire Micr. . .	15′. 54″,03	
Sept. 3, D. O. G. Micr. . .	15′. 56″,01	15′. 55″,4.
Wire Micr.	15′. 55″,9	
A set of ten, all agreeing on the sextant, on the limb, and arch of excess	15′, 55″,0	
Sept. 11, I. T. S. V.		
D. O. G. Micr. 3. 5. 1. 19.	15′. 56″,54	15′.57″,4.
Error of V. + 2		
R. D.		
☉. S. Dr. W. Micr. 15. 41.	15′. 57″,1	
Sextant, 15′. 55″. + 3″. . . .	15′. 58″,0	
Sept. 17, R. D's.		
Dr. of ☉. 15. 43. W. Micr.	15′. 58″,3	15′ 58″,9.
Sextant, ...	15′. 59″,3	
Sept. 27,		
D. O. G. Micr.	16′. 1″,85	16′. 1″,7.
Wire Micr.	16′. 1″,95	
Sextant + 3″, ...	16′. 1″,70	

Observations of the sun's diameter.

Oct. 4, 1794.	S. Dr. ⊙.	Semidiameters of ⊙ as given in the Nautical Almanack,
D. O. G. Micr.	16′. 2″,76	16′. 3″,6,
Wire Micr.	16′. 4″,1	
Oct. 6,		
Sextant ⊙ 3, ...	16′. 3″,0	

The wire micrometer measures, taken from this time till the next vernal equinox, are omitted; inasmuch, as being taken nearly in a vertical circle, the excess of the effect of refraction on the sun's L. L., required a correction from the tables of refraction; which is liable to some degree of uncertainty at low altitudes. They were found, however, to agree very nearly.

Dec. 14. By a set of measures of the sun's diameters, on the limb and arch of excess, taken, with great care, parallel to the horizon, images extremely distinct, and no discernible spring whatever in the index.

⊙'s S, Dr. + 3″ =	16″.18″,0
Dec, 15,	
D. O. G. Micr.	16′. 17″,83

Dec. 29,		Semidiameters of ⊙, as given in the Nautical Almanack.
D, O. G. Micr.	16′ 18″,9	1′. 19″,2.
Sextant + 3″, ...	16′ 18″,0	

Feb. 16, 1795.		
D. O. G. Micr.	16′. 13″,45	16′. 13″,7.
Set of good observations with sextant, ...	16′. 13″,0	

Observations of the sun's diameter.

1795.

March 30th and 31st., Day very favourable; various sets of measures taken with divided object-glass and wire micrometers. The extremes of the divided object-glass micrometer measures never exceeding 1". Those of the wire micrometer 0.

	S. Dr. ☉.
D. O. G.* Micr.	16'. 2",45
* Wire Micr.	16'. 1",9

June 8th. The two different micrometers were applied to the 42-inch achromatic telescope, and the scales verified.

		Semidiameters of ☉, as given in the Nautical Almanack.
Same day,		
D. O. G. Micr.	15'. 46",95	15'. 48",1.
Wire Micr.	15'. 46",45	

June 9,		
D. O. G. Micr.	15'. 46",95	15'.47",4.
Troughton's sextant + 3",	15'. 46",0	

June 15,		
D. O. G. Micr.	15'. 45",9	15'. 47",4.
Wire Micr.	15'. 46",7	

June 19. The measures, with the different micrometers, were taken with the greatest care; and a mean of internal and external contacts, of the sun's limb to the micrometer wires, was used as the measure of the sun's disk by the wire micrometer.

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* This curtation of the sun's semidiameter is the effect of the difference of refraction of the L. L. of the sun from the upper,

1795.

Observations of the sun's diameter.

	S. Dr. ☉.	Mean apogeal semidiameter of the sun.	Semidiameters of ☉, as given in Nau. Alm.
D. O. G. Micr.	15'. 45",26	15'. 46",38	15'. 47",0.
* Wire Micr.	15'. 47",505		

The sextant, on June 25th, shewed, from a careful set of measures, the apogeal semidiameter of the sun, 15'. 44".

Determination of the sun's apogeal diameter.

On attending to the difference of the sun's apogeal semidiameters, as shewn by the divided object-glass micrometer, and the wire micrometer, I had recourse to some former astronomical records on this subject. By referring to De la Lande's Astronomy, article 1387, I find, that, in the year 1758, De la Caille observed the apogeal semidiameter to be 15'. 47".2; and that De la Lande, in 1760, made it 15'. 45"25.

These two measures happen to correspond so exactly with mine, as made with the different micrometers, that it may be a matter of some consequence, to inquire, what kind of micrometers they used to deduce their respective semidiameters.

It is unnecessary to extend these observations any farther. I shall, therefore, only add to this paper, that it will appear, by comparing the divided object-glass micrometer's measures of the sun's diameters, of Decembers 15, 1794, and of June 19, 1795, that the difference of the perigeal and apogeal diameters of the sun was found to be 65", 14. De la Lande found this difference 64",8. but he calls it, in round numbers, 65".

Note. Where no notice is taken of the time of observations, it is to be understood they were taken very near to noon, and as soon after each other, as micrometers could be changed.

The originals of these observations, and several others, are

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* This measure comes nearer to the calculated apogeal semidiameter of the sun than the former; but as, at the making of these observations, the state of the air caused the sun's limb to undulate, perhaps the divided object-glass micrometer, having a much greater magnifying power, than was used with the wire micrometer, its observations may have been rendered more uncertain.

to be seen in the registry of observations kept at the Observatory, Armagh, for the years 1794 and 1795.

I have the honour to be,
Dear Sir,
Your faithful and obedient Servant,
JAMES A. HAMILTON.

Observations on the Metallic Composition for the Specula of reflecting Telescopes, and the manner of casting them: also, a Method of communicating to them any particular Conoidal Figure: with an Attempt to explain on scientific Principles, the grounds of each Process: and occasional Remarks on the Construction of Telescopes. By the *Rev.* JAMES LITTLE [*]

Excellence of the Reflecting Telescope.

THERE are but few things produced by the united effort of mechanical artifice and intellectual labour, which have done more honour to the ingenuity and invention of man, than the reflecting telescope; which has many advantages over any of the dioptrical kind, notwithstanding their improvement by acromatic glasses. It will bear a greater aperture, and may be made to magnify more, (as being more distinct,) in proportion to its length, than the others, as they are at present made; and its dimensions and powers are unlimited. What its excellence is, especially the Newtonian construction of it, has been proved by Dr. Herschell, to his own honour, and that of the age, and country, and patronage, which encouraged his labours. Accordingly, the persons, eminent for science and mechanical ingenuity, appear to have felt a peculiar and disinterested pleasure, in contributing to its improvement: and the late discovery of a metallic composition for the mirrors of it, which will bear as high a polish as glass, reflect as much light as glass transmits, and endure almost equally well, without contracting tarnish, is a farther encouragement to prosecute its improvement to perfection.

Particularly the Newtonian.

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[*] Irish Transactions, Vol. X.

Introduction respecting its Structure.

Among others, I had formerly, from admiration at its contrivance, bestowed some attention on the mechanism of this instrument: and, as it would have spared me some expence of time and trials, if any other person had previously suggested to me the hints, which I am to relate; I imagine they will be of use to others, in directing or assiting the course of their labour, in the same pursuit. I had also taken some pains, to understand the merits of the different constructions of this telescope: but, as this inquiry ended in a conviction, that the Newtonian form of it is the most perfect that can be hoped for, (it being the nature of its great author to persevere in his researches, till he had arrived at a complete solution of his doubts, and comprehension of the subject;) so I have only to report what resulted from my experience in the mechanical fabrication of it, as to the method of casting the mirrors, and communicating to them the proper figure.

On the nature of the Metal.

Before I had heard of the improvements of the Rev. Mr. Edwards, in the composition of the specula for telescopes, I had made many experiments myself with that view; which lead me to give full credit to his report of the superior excellence of that composition which he recommends: because I had found, that the qualities of hardness, whiteness, and indisposition to contract tarnish, necessary to a speculum, could not, by any admixture that I could hit upon, be produced, unless the metal were so highly saturated with tin, as to be excessively brittle; and because I found that this brittleness, however inconvenient in some respects, was necessary to render it susceptible of the highest polish: for no metal yet known, except steel, (which, from its disposition to rust, is unfit for this purpose,) will take as high a polish as glass will, unless it be more brittle than glass. And indeed this property is common to all substances which we know, that are capable of such polish: they must be very hard, and, as such, brittle; for the polishing powder employed would stick and bed itself in any soft metal, instead of cutting and polishing it.

Silver renders it soft; but the trials were not extended.

From the result of my trials, I contented myself with the composition mentioned hereafter, as being in every respect sufficient for the purpose, and inferior to none in whiteness, lustre, and exemption from tarnish: for, as to the addition of silver, I found that, when used even in a very small quantity, it had an extraordinary property of rendering the metal so soft, that I was deterred from employing it: and unless it shall be found that, without this effect, it makes the metal less porous than otherwise it might be, or less frail and brittle, I am certain that it may, in every other respect, be dispensed with. I had no opportunity to try it, in the precise quantity Mr. Edwards recommends, (though I did so before, in very nearly that proportion,) since I first saw his memoir on that subject. Sir Isaac Newton made trial of a very small portion of it, and found the same effects from it as I experienced: but it is possible, that, if it were added in the just proportion discovered by Mr. Edwards, it would be an improvement, and useful ingredient, in the composition *.

The metal liable to tarnish from imperfect casting.

I must observe here, that a metal, not liable to contract tarnish from the air, is otherwise susceptible of it accidentally; when there happen to be minute holes in its surface, caused by the air, or sand, &c. in casting it. Such cavities will be filled with the dust, or rusty solution of the brass, in grinding; which will, in time, become a sort of vitriol, and act on the contiguous parts of the speculum, producing a canker in it, which will spread, in form of a cloud of tarnish, around each cavity. In such a case, to prevent this, I would advise, to lay the mirror, as soon as polished, in warm water, and, after drying, while it remains heated, to rub it over with spirit-varnish, from which it may be cleansed, by a piece of fine linen dipped in spirit of wine. The varnish will remain in the cavities; and, by defending the impurities in them from the action of the air, will probably preserve them from becoming corrosive to the metal.

The Composition, copper, brass, tin, silver, and arsenic.

From numerous experiments, of the qualities of different compositions, made by several persons, it appears, that no combinations, of any other metals or semi-metals, are fit for specula,

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* Having read somewhere, that zinc and gold made the best speculum-metal, I tried it; and found, that the zinc was sublimed from the gold in fusion, and arose to she top in the crucible, forming a white, hard, spongy mass. The metal, called tutanag, is fit for specula, when melted with tin; but I am certain, that what I procued, under the name of tutanag, was a mixture of brass and copper, &c.; for the zinc, in the brass, rose from it, during the fusion, in white flowers.

except those of copper, brass, tin, silver, and arsenic. I tried no semi-metal, except the latter, which whitens copper, and unites intimately with it: because it is stated, in the treatise of the *Art of Assaying*, by the observant and accurate *Cramer*, that all the semi-metals rise in flowers, during the fusion: which would certainly make the metal porus. On this account, I would have rejected the brass, because of the zinc contained in it; but that it seemed to render the composition whiter, and less apt to tarnish, than it would be without it. It will have little tendency to rise in flowers, if the speculum-metal be fused, with the lowest heat requisite, and if the brass be of the best kind; because, in this, the zinc is more perfectly united with the copper, and both are purer. I used, for this purpose, the brass of pin-wire: and, because the quantity of it was only the one eighth part of the copper employed, which, I imagined, would receive too fierce a heat, if put alone into the melted copper; I first added to the brass, in fusion, about an equal quantity of the tin, and put the mass cold into the melted copper; supplying afterwards the remainder of the tin, and then the arsenic; the whole being generally in the following proportion: viz. 32 parts best bar copper, previously fluxed with the black flux, of two parts tartar, and one of nitre, 4 parts brass, 16½ parts tin, and 1¼ arsenic. I suppose, with others, that, if the metal be granulated, by pouring it, when first melted, into water, and then fused a second time, it will be less porous than at first.

The brass.

Proportions adopted.

In this process, whatever metals are used, and in what proportions soever, the chief object is, to hit on the exact point of saturation of the copper, &c. by the tin. For, if the latter be added in too great quantity, the metal will be dull-coloured and soft; if too little, it will not attain the most perfect whiteness, and will certainly tarnish. It is too late to discover the imperfections of the metal, after the mirrors are cast and polished; and no tokens given of them (that I know) are sufficiently free from ambiguity. But I observed the following, which proved, in my trials, at first view, indubitable marks of the degree of saturation; and I think it fit to describe them particularly, as they have not, to my knowledge, been noticed by others.

How to determine the Saturation of the tin and copper.

When the metal was melted, and before I poured it into the flask, I always took about the quantity of an ounce of it, with a small ladle, out of the crucible, and poured it on a cold flag; and observed the following appearances:

From a cast Specimen.

First. If the metal assumed, in cooling, a lively blue, or purple colour, commonly intermixed with clouds, or shades of green or yellow; and if, when broken, the face of the fracture exhibited a silvery whiteness, as bright and glistening as quicksilver, without any appearance of grain, or inequality of texture; then the degree of saturation of the metal, with the tin, was complete and perfect.

And the fracture and colour.

Secondly. If the surface of the metal became of a dun or mouse colour, and especially if of a brown or red; and, when broken, the fracture exhibited a more yellow, or tawny hue, than that of quicksilver; then the quantity of tin in the composition was deficient, and it was necessary to add more.*

Thirdly. If the colour was an uniform dull blue, like lead, where broken, discovered a dull colour, with a coarse grain, like facetts; the due saturation was exceeded, and there was an over proportion of tin in the metal.

Explanation of the colours.

These colours would be more distinct, if a small quantity of the metal were cast in a flask, which had been previously smoaked, by a candle, made of resin mixed with tallow; in which way I used to prepare the moulds. I attribute the formation of the colours to this: that, as the calx of every metal has its own peculiar colour, so, the heat of the melted mass, calcining some of the particles on its surface, which are in contact with the air, these display the colour of the calces of those ingredients which prevail in the composition. Whence, it may be expected, that, if the copper is the redundant metal, the mass will exhibit a reddish tinge, which is appropriate to the calx of copper; and, if the tin be prevalent, a blueish die ought to appear. Either of these colours, therefore, appearing unmixed, shews the redundance of that metal, to which each belongs. And, as brass, when cast alone, has always a yellow tinge, so, when these three colours are exhibited in a cloud-like mixture, they shew an equality and due proportion of their respective metals in the composition. When too large a mass of the metal is cast together, its intense and lasting heat calcines the surface so deeply, as (when exposed to the air) to obscure the colours; so that a small quantity will best serve to exhibit them.

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* This can always be done by degrees, and without any trouble, till the point of saturation is found; whereas, if too much tin were added at first, there would be a necessity for melting more copper separately, and repeating the whole process; and different specimens of copper will require different proportions of tin; so that the due quantity can never be known, *a priori*, but on trial only.

Method of casting.

As to the method of casting the mirrors, it has been directed, to leave the ingate, or superfluous part of the cast, so large, as to contain a quantity of metal, equal to that in the mirror itself; which would occasion a great waste of it, and render it not easy to cast, at once, more than one mirror in each mould; and even this might be done so injudiciously, as not to afford security against a miscarriage of the cast. But it will appear, that this great quantity of metal and incommodious manner of casting it, are by no means necessary. However, a judgment cannot be formed, of what may be the safest and most eligible method for casting the mirrors, unless it be considered, what are the circumstances attending this operation, in the case of malleable metals; and how the management of speculum-metal, in this respect, must differ from that of them: since there must be peculiar difficulty in casting, in sand, a metal more brittle than glass.

Large ingate?

Effects of the contraction of fused metals,

When any fused metal is poured into the flask, the external parts of it, which are in contact with the mould, congeal and harden sooner than the internal parts, and form a solid shell, filled with the rest of the metal, in a fluid state. This will, consequently, remain in a state of greater expansion, from its heat, than the external crust; and its particles will, in the act of shrinking as it cools, recede from one another, as being more easily separable, and cohere, on each side, with the particles already fixed and grown solid: by which means a vacuum will be formed in the middle, and this will be gradually filled by the superincumbent metal, which has been later poured in, and remains longer in a fluid state. But, when there is no more metal supplied, the void, which was in this way latest formed, remains unfilled; and then the shell of the metal, adjacent to the vacuum, as yet remaining soft, and unable to bear the weight of the atmosphere, resting on it, sinks, and is pressed down into the vacuum: by which means, a pit or cavity will be constantly and necessarily formed in the face of the cast, in that part of it which was last congealed; which cavity will commonly be larger or smaller, in proportion to the quantity of metal in the cast.

—particularly in speculum-metal.

The event will, in this respect, be the same with speculum-metal, as it is, in the case of that which is tough and malleable: only that, as the former, in cooling, arrives sooner at its natural state of hardness and brittleness, its external solid shell will not bend, but break, and fall into the void part under it; and thus form cracks, or abrupt chasms, in the places, where tougher metals would contract only regular depressions. And also, when the body of the cast is small, or the mould is so damp or cold, as to congeal, not only the surface, but the substance, of the cast too soon, and thus prevent a gradual influx of the fluid metal, to keep the central part as distended, as the exterior shell was, when it became fixed; the farther contraction of the interior parts of this brittle, refractory metal, after it has become solid, will be apt to form rents in it, because its substance will not bear extension, without rupture.

Remedy: by a supply of the melted metal,

It would be an obvious remedy of the above inconvenience, if there could be contrived a reservoir of fluid metal, to descend into the interior part of the cast, and fill up the void made in it, as fast, and as long, as it is forming by the contraction of the metal. Now, this is effected, by having a jet or appendage to the cast, of such a size, form, and position, as will be effectual to retain the metal, composing it, in a state of fluidity; and also to suffer it to descend into the interior of the cast, until all parts of the same become fixed, and incapable of receiving any farther influx of metal. For thus, all the imperfections, that would otherwise be in the cast itself, will now exist only in the appendage to it, which is a supernumerary part, to be afterwards seperated from it. This appendage ought to be of the form of a prism, and as nearly that of a cube, as the operation of moulding it in the sand will permit; for, in this gross shape, the metal in it will be the longer cooling. It should be connected with that part of the mirror, which is uppermost in the flask, and joined to it by a neck, equal in thickness to the edge of the mirror, (but so posited, that the face of the mirror may project a little above it), and, in breadth, about twice the thickness. This neck ought to be as short as possible; i. e. just so as to

—in the form of a prismatic appendage.

permit it to be nicked round with the edge of a file, in order to break off the prism from the mirror when cast: for thus the heat of the large contiguous body of the prism will keep the neck from congealing; which, if it happened, would stop the liquefied metal, in the prism, from running down into the mirror. And, to prevent this, the prism ought not to form directly a part of the main jet or ingate, by which the metal is poured into the flask; for so the jet would cool sooner than the large mass of the mirror, and bear off the weight of the atmosphere, which ought to press on the fluid metal in the prism underneath, and force it down into the mirror, to fill up all vacuities in it. Both the prism and the mirror, therefore, ought to be filled by a lateral channel, opening (from the principal ingate) into the top of the prism; which latter should be formed broad and flat, and not taper upward, like a pyramid, lest, by cooling where it grows narrow, it might form a solid arch, and oppose the pressure of the atmosphere. When it is fashioned, as here directed, and made of a bulk equal to a third or fourth part of the mass of the mirror, or even a fifth or sixth part, when the mirrors are of large size, there will ever be found, in the top of the prism, after the metal is cast, a deep pit or cavity, which contained the metal, that had ran down into the mirror, after the outer shell of the mirror, and sides of the prism, had become solid and congealed; and the mirror itself will be found perfect, without any sinking or cavity; which could only be formed by an injudicious disposition of the jet or appendage, permitting the metal in it to freeze sooner than the whole mass in the mirror, and thus stopping its descent into it. If several mirrors be cast together, in the same flask, there must be such a separate appendage made to each of them.

The small speculums to be made out of a bar.

In this manner I have (without a failure in any) cast many mirrors of different sizes, and sometimes several of them together in one flask. But very small ones, such as the little mirrors for Gregorian telescopes, cannot be cast in this manner; for their masses being but small, they cool too quickly, to receive any additional infusion of metal; and their outer edges, suddenly forming a solid incompressible arch, the central parts, in contracting towards it on every side, separate, and are rent asunder. And this has happened, even when I cast them in brass moulds made red hot: on which account, I have been obliged to form them out of pieces of the metal, cast in long thin ingots or bars; which, by nicking them across with a file, could be easily broken into square pieces, whose corners could be taken off, and rounded in the same manner.

Reference to Edward's Treatise. See our Journal, quarto series, vol. V.

I do not repeat the other precautions to be observed in this process, which have been already so well and sagaciously described by the Rev. Mr. Edwards: but the circumstances above mentioned, a prudent attention to which, is, in my opinion, essentially necessary to the success of it, are not to be collected from any directions published on the subject that are known to me. And though particular artists may, by large experience, arrive at a sufficient knowledge in this matter, for their own practice; yet, to render that knowledge general, and to contribute, as far as I could, to the improvement of this instrument in any hands, being the design of this essay, I thought it necessary to state the above particulars fully; though I doubt not that these, as well as other matters of moment in the operation, are known to many, who chuse not to make them public. Thus the great skill, in the construction of the telescope, acquired by Mr. Short, seems not to have been transmitted to any successor.

Figuration of the mirrors.

I come now to speak of the most difficult part of the mechanism of this instrument, that of communicating a proper figure to the mirrors; on which depends the powers of the telescope, when its dimensions are given: for the manner of polishing them, to the highest degree of lustre, has been already well understood and described. They who have tried this part of the work, and know how inconceivably small is that incorrectness of form, which will produce grievous aberrations of the rays of light, will, I am sure, readily subscribe to the assertion, that '*hoc opus, hic labor est.*' Methods have indeed been proposed for accomplishing it; but not a single hint given, that I know, of the *modus operandi*, or the grounds of these methods: insomuch, that, when I first tried to polish mirrors, I had no idea why any figure of them, different from that of a sphere, should result from the modes of polishing recommended. But, on my making the attempt, in the ways proposed by Mr. Mudge and by Mr. Edwards, I was surprised to find, that sometimes a spheroidal or other irregular figure, and sometimes (though rarely) a conoidal one, was produced by each: the cause of either being to me then unknown; and disappointment or success appearing to depend on mere accident, and not on the degree of pains and accuracy used in the process.

The methods of Mudge and Edwards vary from the properties of the pitch.

At length I began to suspect, that these variations, in the event of the process, (which will be hereafter accounted for,) arose from some property, not adverted to, in the pitch that covered the polishing tool; which material has been generally used for this purpose, of communicating a proper figure, as well as a high polish, to the mirror, since it was first recommended by Sir Isaac Newton; being commonly spread on the polisher, to about the thickness of a crown-piece, and then covered with the polishing powder: (the manner of doing which I suppose the reader to be acquainted with, as also with what has been made public on the subject, by Messrs. Hadley, Mudge, Edwards, &c.;) and I was confirmed in my suspicion, from the following reasons, after I had found them approved by many repeated and diversified experiments.

The pitch, though hard, is yielding.

Pitch is a soft unelastic substance, which, as such, will suffer a permanent change of form, when it is made to sustain a degree of pressure sufficient to communicate an intestine motion to its particles: and this property directs us to consider, what may be the effect of the pressure of the mirror on it, when spread on the polisher, as to the figure it may then gradually acquire, during the operation of polishing, and the resistance and friction it will oppose to the mirror: for, by reason of the tenacity of its substance, it will resist a certain degree of pressure, without change of its form, but will yield to a greater pressure. But it is by its resistance the mirror is worn down and polished; if, therefore, that resistance be not uniform and equal, on the whole surface of the polisher, neither will the abrasion of the mirror be equal in every part; the consequence of which must be, that both will degenerate from an uniform curvature, i. e. from a spherical figure; the mirror from unequal friction, and the polisher from its mobility, by which it will adapt to the successive alterations produced in the figure of the mirror; their mutual action and reaction inducing a change in both.*

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* This change, however, being so little, as to be imperceptible by the senses, and, in the imagination, referable to various other

The law of its giving way.

As the pitch is (in our present inquiry) to be considered as an homogeneous substance, we must suppose, that its resisting force, as well as that of the pressure of the mirror on it, are uniformly diffused over the surface of the polisher: and, from hence, it may not, perhaps, be easy to conceive, how the surface of the mirror could sustain from it any inequality of resistance and friction. In fact, these would be equal and uniform, in every part, if the pitch were a substance, either of perfect hardness, or perfect fluidity: but it will hereafter appear, that its consistence must not be so hard, as to render it incapable of any change of form: but, on the contrary, so soft, as to yield, in a small degree, to the pressure of the mirror: at the same time, opposing a resistance, sufficient to wear down and polish it: and the inquiry is, how that resistance is modified.

Explanation. It recedes uniformly,

Bodies of perfect hardness, such as glass, flints, &c. will not admit a total intimate change of their form, in all their dimensions, without a dissolution and permanent separation of all the particles composing their masses, (except when they are brought to a state of fusion by heat). But soft, viscid, semifluid bodies, such as lead, pitch, &c. will suffer such change, preserving the cohesion of their particles, yet, at the same time, undergoing a general intestine motion of all the par-

causes, it becomes necessary, in order to establish the true cause, not only to deduce its existence and effects solely from reasoning on physical principles, but also to obviate other different conjectures that might be formed, by stating fully those circumstances that take place in this operation; and which, indeed, are necessary to be clearly understood in judicious practice. Both these ends cannot be answered, in a disquisition new and intricate, without a minute explanation: and this, I hope, will be received as my apology, for the prolixity of this account, which I would gladly have curtailed, if I knew how to do so, without making it less intelligible or useful to the practical optician. This class of readers will forgive any diffusiveness on a mechanical subject, if the perusal may tend to spare them the greater labour of fruitless experiments; or afford any hint towards conducting them more judiciously: and as for their use this paper was designed, I have adverted to such various matters as I thought most worthy their attention; and which yet have not been so fully and familiarly explained by others, as they ought to be, for the instruction of an artist.

ticles among themselves: so that the coat of pitch, pressed, on each side, between the parallel surfaces of the mirror and polisher, will, by their force, be equally extended laterally in every direction; by which an equal quantity of motion will be communicated to all its particle; since no particles, except those at the extremities, can move, without protruding others, and these, the rest, successively, as if the mass were a fluid body.

—but with different resistance at different parts of the surface.

But, though all parts of the surface of the polisher receive an equal pressure and motion, all do not exert an equal degree of resistance to that pressure: for those parts, that cannot move without displacing and overcoming the resisting tenacity of a greater quantity of the surrounding mass of pitch, than other parts do, must oppose the greater resistance to the mirror, as having that of the other parts superadded to their own. For ascertaining this, the force impressed, and the quantity of pitch, confining any annular tract of the polisher, should be computed. In the present case, where the coat of pitch is a thin equal statum, of circular form, we need regard only its superficial dimension, and consider all parts of it as alike situated in the above respect, which are equidistant from the center, or from the outer edge of the polisher.

Deduction of the quantities of reaction or pressure.

To this purpose, let the surface of the polisher be conceived to be composed of an indefinite number of concentrical zones or annuli. Each of these will sustain an uniform pressure, from the mirror, proportional to its area, because, the force impressed on the mirror, and its attraction to the polisher, is equally diffused on it. The areas of these annuli, taken separately, are the differences of the two circles, whose peripheries inscribe and circumscribe each of them; and they are consequently to each other, as the differences of the squares of their diameters, or as those of their radii; and the series of them taken, in order, from the center to the extremity, are strictly as a rank of figurate numbers proceeding from unity, viz. the odd number 1, 3, 5, 7, &c. But, since their breadth is supposed to be infinitely small, they may be taken as proportional to their mean diameters or radii, i. e. as their distances from the center of the polisher; which distances will, therefore, represent the pressure on each annulus, and the quantity of motion communicated by that pressure; seeing it must be, as the number of particles the annulus contains that are moved; i. e. as its area.

This resistance may be raised,

But the resistance to the force impressed on any annulus, being as the quantity of pitch to be put in motion by it, will be different, not only as the annulus is nearer to, or farther from, the margin of the polisher, but different, also, as this has either one margin only, or two, i. e. when the polisher is entirely covered with pitch, or when it has a space left uncoated at the middle: which latter always is, and must be the case, when the great mirror of the Gregorian telescope is to be polished, which has a perforation at its center.

—by taking away some of the surface of the pitch at proper places.

First. When there is no vacant space in the middle: the resistance to the several annuli will be as the circumambient spaces only; because, the pitch not being compressible, it is only into these, and not towards the center, it can, in yielding to the force or weight of the mirror, extend itself, by lateral motion: and the space, surrounding any annulus, is the difference between the circular area of the polisher, and that inscribed in the annulus: and is, relatively to the rest, measured by the difference of the squares of their radii, viz. of the distances of the edge of the polisher, and that of the annulus, from the center. But since, in this case, the bodies (of pitch) are unelastic, there can be no augmentation of motion; nor can the quantity of motion and action communicated, and, consequently, the resistance to it, and reaction, exceed that which is impressed: on which account, I imagine, that the resistance to the several annuli is to be taken as proportional to the pressures they sustain, and measured by them, i. e. by their magnitudes or areas, or the number of particles in them, to which a motion is imparted; which were stated to be as their respective radii or distances from the center: and, consequently, I suppose the resistance to be the inverse of this, or as the distances of the annuli from the outer edge of the polisher; which distances measure the direct resistance, or the quantity of pitch, to which equal motion, with that in the respective annuli, is communicated.

Effect of a central hole,

And from hence it follows, that, if a mirror, previously ground to a spherical figure, were to be polished on such a polisher as this: the resistance and friction of the pitch, being greatest, and increasing to a maximum at the center, and diminishing towards the extremity, would wear down and polish the mirror, most in the central part, and least towards its edges; thus giving to it a curvature, the reverse of a conoid, which it ought to have, and which it can never at first acquire correctly, by any other mode of polishing, but that of wearing it most down (and thus reducing its curvature), towards its extremities.*

Secondly. When there is a hole made through the center of the polisher, or a void space left there, uncoated with pitch.†

—shewn in the figure produced on such a polisher.

In these circumstances, the pitch will have liberty to expand itself (when yielding to the pressure of the mirror), towards the center, as well as the edges of the polisher: and, as the resistance and friction, in any annular tract of it, is as the direct extent of pitch, bounding it on either side, it follows, from what has been laid down, that it will encrease in any part, as the distance of the same annulus encreases, from each extremity of the coating of the polisher; and will be in a ratio compounded of the distances, from the interior and exterior margins of the pitch. So that, if the breadth of the polisher between these margins were, (for example,) 5 inches: then the pressure and friction in the middle tract, equidistant from the outer and inner edges, would be, to that prevailing at the distance of half an inch from either margin, as 6¼ to 2¼, (nearly as three to 1;) and the same, at proportionate distances, in polishers of any other size; which unequal pressure could never produce, in the mirror, a regular curvature of any species; and, in the spaces nearer to the margins, the inequality of pressure would be still greater. Whence may be conceived the impossibility of figuring mirrors correctly, on polishers disposed in this manner, without some remedial contrivance; whether the face, or area of them, be of a circular shape, as directed by Mr. Mudge and others, or oval, as proposed by Mr. Edwards: for the mirror would be thus least reduced, and left of a spherical form, at the middle and edges; and be worn down, and hollowed into a different and irregular curvature, in the intermediate tract.

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* It will be hereafter shewn, for what particular purpose, solely, such a polisher may be employed.

† There ought always to be a hole made through the polisher to prevent the confinement of air or water, near the centre of it.

Remedies.

For these inconveniences, however, arising from the unequal friction of the polisher, there are the following easy and adequate remedies; which will, in the sequel, be more fully explained, and applied as in practice, to effect the degree of curvature, or any correction of the same, which may be requisite.

Make the polisher larger than the mirror.

First. Since the curvature of the mirror ought to be gradually reduced towards its edges, which can only be effected by an increase of friction in the corresponding part of the polisher; and that this latter effect is to be produced in any part of it, by enlarging the surrounding coat of pitch: it follows, that, for this purpose, the breadth of the polisher must be enlarged above that of the mirror; and this in the same degree, as the curvature of the mirror is to be diminished: so that the polisher is to be of greatest breadth, for a mirror of an hyperbolic, and least, for one of a spherical figure. This, however, is to be done, under the limitations hereafter mentioned.

Contract the center hole.

Secondly. To preserve the regular gradation of curvature towards the middle of the mirror, the uncoated space, at the center of the polisher, should be contracted to a certain limit, which will be defined; though, for the reasons above mentioned, it can never be filled up altogether.

Cut out some of the face of the polisher where the reaction is greatest.

Thirdly. Where the resistance and friction of the pitch, in any tract on the face of the polisher, is computed as above, or found in effect, to be too great; it may be lessened and regulated, in any degree, by cutting, out of that part of its surface, some of the pitch, at proper intervals, in narrow channels or furrows: the number and depth of which ought to be proportioned to their distance from the edges of the coat of pitch directly, and to the reduction of curvature, proper to the corresponding parts of the mirror inversely, and should be in a ratio compounded of both; for, by these cavities, the continuity of the pitch being dissolved, its resistance, depending thereon, may be modified at pleasure.

In this way, the small, as well as the large speculums, may be duly figured.

In this manner may the polisher be so disposed, as to communicate a correct figure to large mirrors, and even to those of smallest size. Now, whatever success may have attended the efforts of other persons, in communicating a proper figure to the *great* speculum, (especially Mr. Short, whom I have manifold reasons for believing to have been among the most eminent opticians, as well as artists, that have laboured in the improvement of this instrument;) I have not heard, that any method has been proposed, of communicating, to the *little* mirror of the Gregorian telescope, any other than a spherical form, which yet may in this manner be done. And it must, in this telescope, be a thing most desirable to accomplish; especially when its size and aperture is so great, that it would be difficult to impress, on the extensive surface of its great mirror, (merely by the small alteration of figure, which could be produced, in the delicate operation of polishing,) the degree of change, from its prior state of spherical curvature, which would be requisite; since the defect of form, in this mirror, may, in these cases, (as will be shewn,) be easily compensated, in the figuration of the little mirror. For the greater size of this latter, in such instances, will render it capable of more steady handling and motion, and more equal pressure; and so more manageable, and susceptible of a correct figure, in proportion as the encreased magnitude of the great mirror renders it unmanageable: which is, plainly, a great advantage, in the fabrication of this telescope; whose mirrors will thus, in the cases where it is most especially necessary and desirable, admit mutual correction and compensation for each other's defects.

Difficulties of the processs.

The principles, or physical causes, operative in this process, as above stated, seem to be incontrovertibly evident; and, as I am not aware of any paralogism admitted in the reasoning upon them, I must suppose, that a mode of operation, conformable to these principles, is the thing chiefly requisite to ensure success. In this view, I have attempted to conduct the process; and, as the almost insuperable difficulties attending it are felt, even by those whose inventive powers and resources ought to afford the highest hopes of accomplishing the object, and yet disappoint them in their attempts at high perfection;* so I, among others, may be allowed to state the difficulties, that, to my apprehension, occurred in the enterprize, and to obviate objections; as, from hence, there may be suggested some hints, to facilitate or abridge future labour [illegible] others, or to prevent hopeless trials.

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* Sir Isaac Newton, who had himself laboured in this undertaking, of polishing the concave mirror of his own telescope, and with such talents for the work, and such success, as to discover that method of doing it, which has, to this day, been followed, observes, (to use his own words) that "optick instruments "might be brought to any degree of perfection imaginable, pro-"vided a reflecting substance could be found, which would po-

Arguments and inferences respecting the process of polishing mirrors.

I must observe, then, that different effects must necessarily follow, from using, in the process of polishing, pitch of a softer or harder consistence. If the pitch be of a temper quite hard and unyielding, no part of the surface of the mirror can be made to suffer a higher degree of friction than the other parts of it, unless these latter parts be elevated and detached from the face of the polisher, and disengaged from contact with it; because, in this case, both mirror and polisher are supposed to preserve their general shape regular and unaltered; and therefore, the contact, and, consequently, the friction, must be either complete and equal, on the whole surface, or none at all. For, if we suppose, that, by the wearing down

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" lish as finely as glass, and reflect as much light as glass trans-
" mits, and the art of communicating to it a parabolic figure be
" also attained. But there seemed (*said he*) very great difficulties,
" and *I had almost thought them insuperable*, when I farther
" considered, that every irregularity, in a reflecting superficies,
" makes the rays stray five or six times more out of their true
" course, than the like irregularities in a refracting one; so that
" a much greater curiosity would be here requisite, than in figur-
" ing glasses for refraction. . . . . &c.

" But having afterwards thought on a *tender way of polishing*,
" proper for metal, whereby, as I imagined, *the figure would also*
" *be corrected to the last*, (i e. to the utmost) I began to try what
" might be effected in this kind; and, by degrees, perfected an
" instrument . . . . &c . . . . and afterwards an other one."

The tender way of polishing, which Sir Isaac Newton here mentions, was (as he afterwards described in his Optics,) to cover the polisher with pitch: and he declares, that he imagined the *figure*, as well as the *polish*, would by means of this, be perfected. I cannot help thinking, that this extraordinary man, who was born to anticipate others in invention, as well as discovery, had the same ideas as are here detailed, though he did not explain, nor, perhaps, succeed in, the application of them in practice: for he states, (in his Principia) that a spherical mirror will reflect the oblique pencils, issuing from the extremities of the field of view, as truly as a parabolic one, and seems to despair of effecting a more correct figure.

Arguments and inferences respecting the process of polishing mirrors.

of the mirror towards the extremities, it is made gradually to change its spherical form, the part of its area, so abraded and diminished, cannot subside into a state of actual contact with [illegible] polisher, unless the other parts of it are elevated and disengaged from the polisher, at the same time; or unless it may be imagined, that the particles, worn off the mirror by friction, are applied and adhere to the corresponding parts of the polisher, so as to raise and augment its surface, just as much as that of the mirror becomes depressed and reduced. If this effect could be supposed to take place, it would follow, that, in every variety in the direction of motion in the mirror, the friction must tend to wear down the edges, rather than the middle of the mirror; because the motive force is always applied to a part of the handle to which the metal is fastened, raised more or less above the surfaces in contact. The effect of which must be, to communicate to the foremost or advancing half of the mirror's surface, a pressure downward, on the face of the polisher, equal to the force expended in moving the mirror forward; and thus to abrade and reduce the several parts of the mirror's surface, proportionally to their respective distances from the center; by which its curvature will be made to approach to that of a parabola, by its wearing down most towards the edges: and this, weather the motion be conducted in lines diametrically across the polisher, or with round strokes; so as that its center should describe, every time, a little circle, about the center of the polisher. This is, however, entirely on the supposition, that the edges of the polisher become raised, by the adhesion of the dust worn from those of the mirror: for, if this were not the case, but that the polisher were to retain its spherical form, while that of the mirror was altered, the contact could not be general between two surfaces of dissimilar shape. If these adhered together in one part, they must be dissevered in another: and the force, necessary to seperate them in this latter part, which can never be greater than that required to move the mirror forward, must yet be more than equal to the force of cohesion, in the part of the mirror, which, in each stroke, is to be disengaged from the polisher. This pressure is found, in the case of bodies in contact, to be incomparably greater than the weight of the atmosphere, which is equal to about seventeen or eighteen pounds on every square inch of the surface of the mirror:

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and, when this latter is brought so near that of the polisher, as to suffer friction from the powder bedded in it, their mutual attraction will amount to a much greater force than is requisite to move forward the mirror: no part of which can, there[illegible] be disengaged from the polisher, nor, consequently, be unequally worn down, so as to produce, in its surface, a form different from a spherical one, or from that of the polisher.

This reasoning and conclusion will equally stand, whether it be supposed, that the force of cohesion is confined to the very surfaces in contact, or extends to a little distance from them, diminishing in the duplicate, or any other ratio of that distance; and that the bodies are not wholly removed out of the sphere of attraction when there is a small interval between them. For, as this force is greatest at the very surface; so, the bodies in contact cannot be disjoined at all, to the smallest distance, but by a force superior to the whole cohesive force.

It may, perhaps, be imagined, that the pressure of the atmosphere ought to be taken into consideration, and be added to the force of cohesion, which keeps the surfaces in contact with each other. But this pressure acts as much upon the coat or plate of water, which must be interposed between the surfaces of the mirror and polisher, as upon these surfaces themselves: and, because the pressure upon any part of a confined fluid, is propagated to the whole of it, in every direction; so, the weight of the atmosphere, resting on the edges of this fluid plate, tends as much, by the interposition of the same, to buoy up, and force assunder, the surfaces resting on it, as it does to compress together these surfaces, by its action on themselves; and exerts itself equally to prevent their approach on one side, as their recession on the other. I conceive the agency of these forces to be this: that the plate of water is so strongly attracted by she surfaces nearly in contact, as to be kept from running off, and has its outer edge exposed to the weight of the air; whose pressure is thus communicated to all the particles of the water, and, by its mediation, to the contiguous surfaces of the mirror and polisher. And, though all these are really compressed together, by the surrounding atmosphere, yet I conceive that this does not hinder their gradual separation from being effected: because, as fast as that separation takes place on any side, the air and water rush in between the surfaces, to fill up the vacuity, as

Arguments and inferences respecting the process of polishing mirrors.

it is formed; and no farther resistance arises to their disjunction, than what is owing to the viscidity of the fluid interposed, and to the force of cohesion; which latter acts, in this case, quite different from any external force of compression; and prevails, as I apprehend, to a small distance from the surface, diminishing in the ratio of some high power of that distance.*

And hence I suppose, that the weight of the atmosphere is wholly inefficient, in keeping the mirror and polisher in mutual coherence, when any liquor of perfect fluidity is between them; and that the force of cohesion acts alone to this effect. Accordingly, it is found, that, when the polisher is so much wetted with water, that there is formed a continuous plate of this fluid between it and the mirror, an additional force, sufficient to squeeze out the water interposed, becomes requisite to bring the surfaces into actual contact, and to produce so much friction between them, as will serve to wear down and polish the metal; which process will be found, in these circumstances, to advance very slowly and irregularly. And, on the contrary, when so little water is applied to the polisher, that it is only made damp, and scarce wetted, (i. e. when there is not a continuous body of liquid interposed between it and the mirror,) then its contact with the metal will be so intimate and strong, that the latter will polish very quickly. For then their surfaces approach within the sphere of the attraction of cohesion: insomuch that, if all moisture were suffered to evaporate, the mirror and polisher would cohere so firmly, as not to permit any friction, or even a separation of their surfaces, and the polisher would be destroyed; for then the weight of the atmosphere, also, would be superadded, when no fluid is interposed:

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* If it were supposed, that the force of cohesion is confined to the surface of bodies, and acts only in the state of actual contact; it would be hard to conceive, why a drop of liquor should ascend, in a conical glass-pipe, whose narrow end was elevated: since the drop ought, on this supposition, to be attracted as much by the surface below, as by that above it: and its weight ought to make it descend; and there would be nothing to make it spread beyond the space of contact which it occupies: whereas if the attraction extends, directly in right lines, to a distance from the sides of the pipe, the composition of their forces ought to make the drop ascend, and spread itself in its course, as it happens in fact.

all which shew that their cohesion, when a fluid does intervene, is not caused by the pressure of the atmosphere.

Agreeably to this, the sagacious Newton directs, that, towards the end of the operation, no more moisture should be applied to the polisher, than what it will contract, from the operator's breathing on it. Indeed, a person, who has formed a just conception of his genius and intense application of mind and considered the hints and precepts he has given in this work, can hardly doubt, that he could, and, perhaps, would, have furnished a theory of the rules and method of this whole process; had he not imagined it would, at that time, be regarded as a matter of too little importance, to deserve so minute an explanation, which must be necessarily prolix, and seem unworthy of him, who was occupied in more sublime speculations.

Hard pitch will not give a good figure,

From this it follows, that, when the pitch is of unyielding hardness, it will not, in any mode of polishing, communicate to the mirror the desired shape, if the dust worn from the mirror, does not alter the shape of the polisher. And, as this seems not likely to happen, so I was not surprised, that my efforts, to effect the desired figuration of the mirror, by using very hard and refractory pitch, failed of success.

And there is this inconvenience, moreover, in the use of such pitch, viz. that it makes so great resistance to the sinking and bedding of the polishing powder in it that the particles of the powder, however fine it may be, will, on any fresh application of it, or when any grains of it are accidentally dislodged from the pitch, roll about loose on the polisher, and scratch the face of the mirror, so as to destroy the polish before given; thus making any fresh application of the powder inadmissible, unless the pitch were to be softened by heating it, which would destroy its former figure, and render the operation uncertain and tedious.

—nor will it receive the polishing powder.

It was to allow the polishing powder to fix itself, without rolling loose on the polisher, and to suffer all its particles, however different in size, to sink in it, so as to form an even surface, that Sir Isaac Newton, in his sagacity, employed a coat of pitch on the polisher, as a soft substance, that would yield to the powder, when impressed on it by the mirror, and not afford such resistance, as to make it fret the face of the metal; and also as a substance endued with another property equally necessary, that of being perfectly unelastic. For no elastic substance will ever communicate an exquisite polish to a metallic speculum, though it would to glass, crystal, or jewels; because no metal can be cast, perfectly free from small pores: and any elastic substance, if employed to polish it, would insinuate itself, together with the polishing powder, into these pores, and wear down their edges in such a manner, as to convert every pore into a long furrow or cavity: which would occasion the destruction of the whole surface of the metal, as was truly observed by Sir Isaac Newton. And thus it appears, that, to make the pitch too hard and refractory, would be to destroy every property in it, which renders it eligible in this operation.

The polisher must change its figure while that of the mirror changes.

If the positions, before stated, be well founded, it seems to follow, that the desired change in the mirror, from a spherical to a conoidal figure, can only be effected, by a change in the shape of the polisher, gradually accommodating itself to the alteration, produced in that of the mirror, during the process of polishing. Nor, indeed, can it well be conceived, how the mirror could alter its spherical form, if that of the polisher remained unaltered; for a conoid could never, in the usual way, and without a partial separation of the surfaces in contact, be polished on a segment of a sphere, nor even on that of a conoid, if, during the friction of their surfaces, the center, or vertex of the one, were to be moved to any considerable distance from that of the other. So that the strokes, in polishing, must never ultimately be carried so far as to remove the center of the mirror to too great a distance from that of the polisher; even though its surface were so hard, as to preserve its figure unaltered by the pressure of the mirror.*

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* For the several reasons above mentioned, I am inclined to think, it will be very difficult to discover a method, different from that here explained, of communicating, at the same time, a perfect figure and polish to a speculum. It is plain, that Newton could think of no better; though I imagine that, in this instance, he tried his inventive powers with those of Des Cartes, who had published a method (in theory elegantly geometrical) of figuring optic glasses. And I cannot dissent from those, who think this was the method employed by Mr. Short, with such success, in figuring the mirrors of his telescopes; I mean a conduct in the operation, sagaciously adapted to the properties of the pitchy coating of the polisher.

It must be obvious to the reader, that none of the remarks or directions, contained in this essay, can be meant to apply direct-

The pitch should be a little harder than common.

Agreeable to these positions, I found, in my trials of polishing mirrors in the common way, by straight or round strokes of the mirror, on the polisher, that the operation was more easy and successful, when I used pitch of nearly the common consistence, than when I employed such as was made very hard, by long boiling it, or by the addition of much resin. Such softer pitch will admit more than one application of the polishing powder, without scratching the metal, or spoiling its previous polish; by which means, the process will be more expeditious. It will instantly accommodate itself to the successive alterations in the form of the metal; as this, by wearing down towards its edges, gradually changes from a spherical, to a conoidal shape: and it will promote this effect, by opposing a greater resistance to the

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ly to the polishing any speculum, whose magnitude is too great, to admit of being moved on a polisher, of equal size with itself. Where the friction, and force of cohesion, of such large surfaces in contact, and the weight of the mirror, exceed the motive power that can be employed, a polisher, of less extent than the whole surface of the mirror, must be applied, to traverse, in succession, the several parts of it; and the motion must be given, not to the mirror, but to the polisher. Instruments of far less enormous magnitude than Doctor Herschell's great telescope, are *sui generis*, and require particular methods of polishing the mirror adapted to their size. For such, no person should presume to propose any method, which he has not approved in practice: though, as the general principles here laid down, are, with due accommodation, applicable to a polisher of any shape or extent of surface; it should seem, that, if such great mirrors could be polished by a regular and uniform motion, their polishers might be made such segments or sectors, &c. of the area of each respective mirror, and of such breadths in different ports; and the furrows, made in the coating of pitch thereon, of such number, proximity, and depth, as to afford, in the tract of the motion of each part, a degree of pressure and friction, reciprocally proportional to the degree of curvature, proper to each concentric zone of the mirror's surface; which would tend to produce the desired figure, so far as a polisher, covered with pitch, could be made instrumental to this purpose. For though the size and shape of the polisher were to remain unaltered, yet its resistence and abrading power might be considerably modified, by varying the number and depth of the furrows, made in the pitch which covers it. And the effect of a process, thus conducted, will be commensurate to the time it is persisted in.

metal, and greater friction towards its extremities, when its previous disposition on the polisher has been judiciously provided, in the manner before explained.*

Description of the kind of stroke most proper for giving the figure.

But, to fulfil these intentions effectually, a certain kind of motion, of the mirror on the polisher, must be carefully observed, during the operation: for, as the softer pitch will continually yield, and sink under the pressure of the metal; so, the form of the polisher, degenerating in every stroke, must be recovered, and preserved correct. According to the principles before laid down, the face of the polisher must be considerably larger than that of the metal, in order to afford a greater resistance to the speculum, towards its extremities: so that, as the metal covers only a part of the polisher, if the former were to be confined in its motion, the pitch, sinking under it, would expand itself laterally, and become heaped up suddenly, around the tract of the mirror's pressure; which must, therefore, to obviate this, be so conducted, as to traverse, in quick and regular succession, every part of the polisher in order to recover the regularity of its figure as fast as it becomes vitiated. And this is effected in two ways: either by enlarged circular strokes of the metal, brought considerably beyond the edges of the polisher, in order to repress, towards the center, the pitch, which had become raised near its edges, or by straight diametrical strokes, across its surface, in every direction successively: either of which will tend to preserve the figure of the polisher, and, consequently, of the mirror, nearly spherical. As, however, a spherical figure is not that which is ultimately intended, so these modes of conducting the process are to be pursued only till the mirror has acquired a sufficient polish, and a figure nearly spherical: and then, in order to give it a parabolic or hyperbolic shape, the motion of the mirror, on the polisher, should be such, as that the center of it may describe a spiral line round the center of the polisher, by enlarging the circular strokes, till the edge of the mirror arrives at the edge of the polisher; and then contracting the motion gradually, till the mirror returns to the center, in the same spiral course. By which means, any sudden and irregular elevation of the pitch, beyond the place of the mirror, will be prevented: while, at the same time, it will become regularly elevated, from the outer edge, in the form of a conoid, and thus be adapted for communicating the same figure to the mirror.

I have been led to adopt and practise this method of polishing mirrors, by the train of reflections and reasoning herein described, and with sufficient success, for its unreserved recommendation. In one particular, it corresponds with the method published by Mr. Mudge, in the Philosophical Transactions, viz. in the direction of the motion used in polishing the mirror. But this seems to have been prescribed by him, without any respect to the properties of mobility and inequality of friction, in the pitchy coating of the polisher; which things he has not noticed. And yet, as any sort of motion, without a proper regard and adaptation to the qualities of the pitch, would be ineffectual, it is here attempted to supply that defect; because no method can be rightly pursued in practice, nor its success be uniform, nor any figure already given to the mirror be altered, if those artists, who would follow it, are ignorant of the principles and agency on which it is really founded. For, in every process of so subtil and delicate a nature, some untoward accidents and circumstances must occur, which will grow above the control and correction of any person, who is not aware of the secret causes from whence they arise. In such cases, the practice will be as imperfect as the theory is.

Method of taking away part of the face of the polisher.

It has been above explained, how the middle zone, or tract of the polisher, equidistant from its inner and outer edge, when there is a void at the center, will oppose a greater degree of friction to the mirror, than the other parts of the polisher. And, to prevent the unequal wearing of the mirror, by the increased action of this zone, it will be proper, that, agreeable to the methods of prevention of this effect before mentioned, there should be circular furrows indented in the pitch within this zone, more or fewer, according to the size of the mirror, and the designed degree of its curvature; in order that the pitch may subside into the furrows, and thus the resistance and friction in that tract may be diminished. This will be very easily accomplished, by putting the polisher on the arbor of a lathe, and cutting out some of the pitch in circular grooves, with a small sharp and concave turning chisel, wetted with water, in which some soap has been dissolved. And this may be performed and repeated, if necessary, without any injury to the surface of the polisher, if it be previously wetted, to prevent the splinters of the pitch from sticking to it; which may be washed off, by a soft brush or pencil, from the polisher, it being immersed in water.

It is better that the small speculum should be spherical.

Since, in the Gregorian telescope, the defect of figure or curvature, from that of a conoid, in one of the mirrors, may be compensated by a contrary curvature in the other; and since, in either of the mirrors, whose breadth is given, the degree of variation in its figure, from that of a sphere, ought to be so much the greater, as its focus, or radius of curvature, is shorter; it will, on this account, be far more difficult, to effect a proper figure of the small mirror in this telescope, than of the large one; because the former must be a greater segment of the sphere, than the latter. For which reason, instead of making the one of an elliptic, and the other of a parabolic form, I imagine it would (with the exceptions before mentioned) be more proper to rest content with a spherical form in the little mirror, (by which means, several of these latter, being fastened, with cement, beside each other, on the same handle, might be accurately and easily ground and polished together, on one tool and polisher, made sufficiently large); and to employ the great efforts on the large mirror, in rendering it of an hyperbolic form; which is not at all more difficult than it is to make it parabolic: for, on account of the small extent of surface of the little mirror, it is very difficult to govern and regulate its motion and pressure, so as to communicate to it any certain figure, if polished by itself singly; as it must be, when it is to be of any other than a spherical form. Yet, even this may, by an intelligent and dexterous artist, be accomplished, to a considerable degree of perfection, in the manner above mentionned, as I have repeatedly experienced; though the process is much more easy and certain, in figuring the large mirror (under that limitation of its size before intimated): for the greater the surface to be polished is, the less will any inequality of pressure, in the operation, alter the form of the mirror, or the polisher; such inequality, being a part only of the motive force employed; and the more extensive the surface is, the less proportion does the motive force bear to the force of cohesion, which tends to preserve an uniformity of pressure in the mirror, and of figure

—but the conoidal figure may be given.

in the polisher. And I believe it is on this account, rather than that of preventing aberrations of the rays of light, from a supposed spherical shape of the mirrors, that telescopes of greater apertures and foci are more accurate; the larger surfaces of their mirrors having a tendency, during the operation of polishing, to preserve the regularity of their figure. For, let the aperture of a telescope be ever so large, with respect to the focus of the great mirror; yet when the object is very remote, the central part of the field of view (the rays of light from which are parallel to the axis,) ought to appear perfectly distinct, if the metals were wrought up to the correct figure of conoids: and the vulgar doctrine of aberrations, which relate only to spheres, is entirely inapplicable. The only standard, for the measures of the apertures and foci, is the degree of ingenuity in the workman, who fabricates the instrument. There are many defects in figure, besides a spherical form of the mirrors; and it happens but too frequently, that a telescope is very indistinct, from a bad figure of them, though that figure is the nearest to a conoid of any regular curve: for this is often the case, when the central, the extreme, and the intermediate parts of the mirror, successively and separately exposed to receive the light from the object, appear to have the same focus. And this mostly occurs, when the mirrors are small; certain tracts, or portions of their surface, being more worn down, by the grinding or polishing, than others, arising from the difficulty of preserving an uniform pressure during the operation, and, consequently, a regular figure of the polisher.

A larger speculum is most easily figured.

Another method, different from that now described, of communicating to mirrors a parabolic form, has been discovered by the late Rev. M. Edwards, and published in the Nautical Almanack, for the year 1787. He recommends, to make the edge of the polisher the periphery of an ellipse; so that the face or area of it may not be round, but oval: the shortest diameter of the ellipse being equal to that of the mirror; and its longest diameter to be to the shortest, as 10 to 9. And he affirms, that a mirror, finished on such a polisher, will prove to be of a parabolic form; if the process be conducted, by employing, throughout the operation, straight strokes of the mirror, diametrically acros the polisher, in every direction. Now in the method recommended by M. Mudge, whatever kind of motion be used, in bringing the face of the mirror to a polish, the parabolic form is directed to be acquired, only by a circular motion in polishing: Mr. Mudge having declared, that the effect of such straight strokes would be, to produce no other than a correct spherical figure in the mirror. Here, then, are opposite motions, and declared to be productive of contrary effects, proposed by two very intelligent artists, with a view of promoting the same effect; the only difference being this, that, in the one case, the face of the polisher is supposed to be round, and in the other, oval: a difference that a person may well imagine to be (as it really is) of very little importance; and he may be easily led to suspect, that the presumed effect of either mode is only imaginary; that a spherical figure of the mirror has been mistaken for a parabolical one: or that, if the latter has been produced, it may have been, not by method, but by chance; and he may naturally distrust any rule or method advanced for this purpose. Thus, when different instructions are given, by different persons, without any reasons or explanations assigned as the foundation of them, the whole rests on authority; authorities clash, and then the worst may be followed, or all be rejected; and, for want of a guide, an uncertain practice be adopted. It is for this reason, I have judged it necessary here, (as also in former essays, made public,) to be very minute, in attempting to investigate the grounds of any method to be pursued, and the principles of action, in the operation of the instruments I am treating of.

Commendation of the method of Edwards.

I have made a trial of the method of polishing, proposed by Mr. Edwards, with attention to all the circumstances, which he directs to be observed; and, from the result, I have reason to believe, that his method is a good one, and will, if judiciously applied, produce as correct a figure of the mirror, as, perhaps, any other, yet made public. But, whoever will attentively investigate the nature of the operation, will, I think, cease to wonder, that modes of conducting it, seemingly so dissimilar, tend to the same effect, and perceive, that the contrariety is not real, but merely apparent *.

For, in either method, it is not the direction of the motion

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* In the methods of figuring the mirrors, published by Mr. Mudge, and by Mr. Edwards, it is stated by Mr. Mudge, that he frequently, during the process, applied to the polisher a concave

The radial and the spiral stroke compared.

employed, nor the shape of the area of the polisher, which, in reality, produces a conoidal form in the mirror; but a gradual alteration in the curvature of the face of the polisher, by yielding of the pitch, under the pressure. And, therefore, when any part of the area of the polisher, whether it be round or oval, is more extended than that of the mirror; the pitch, moving laterally, will become elevated, and its curvature lessened, in that part. So that, in a polisher of oval shape, whose conjugate diameter is equal to that of the mirror, the pitch will ascend and accumulate, in the part, which lies without the circular area of the mirror, inscribed in the ellipse. The extremities of the mirror will, therefore, be worn down, when each part of them is made in rotation, by straight strokes across the polisher, in the transverse diameter of the ellipse, to traverse that part of it, which circumscribes the circle; and, by such strokes made twice, directly in that diameter, and oftener obliquely, in each rotation of the mirror, as the operator moves round the polisher, during the process, the regular shape of the polisher is preserved. But it is easy to conceive, that the same effects would follow, though the polishing were conducted, not by straight strokes across, but by round strokes, in a spiral direction, as above mentioned. And I am doubtful, to which of these motions the preference should be given; or whether they ought not to be interchangeably used, to produce the most elaborate form in the mirror; as also, whether this method, of Mr. Edwards, is better than the former, by Mr. Mudge, above described. For I have been deprived of leisure and opportunity (by the war, and public troubles, during the French invasion and the rebellion; in which, most of my instruments, for such purposes, were lost, in the plunder and

Doubt which may be preferable.

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tool, which he calls a bruiser; by which he must have preserved or recovered, the figure of the polisher, and, consequently, of the mirror, that otherwise must have become vitiated, by the unequal resistance of the pitch; and Mr. Edwards made furrows in the coating of pitch, or his polishers. It is to these circumstances, and not to the direction of the motion employed, or the elliptic area of the polisher, that, I can think, was owing to the success, attendant on their methods: the bruiser being necessary, to supply the defect of furrows in the pitch; and the oval form not essential, when there were such, duly disposed, and also the polisher of proper size, &c. as here directed.

destruction of my house,) to prosecute the experiments, which might have enabled me to speak with more precision; and which I would have done, from the desire I had, to contribute to the perfection of so noble an instrument as the reflecting telescope.

Both may succeed.

I know, that both methods will, in judicious practice, produce the desired effect; but this effect will be limited, in degree of perfection, and sometimes frustrated, when the causes and circumstances, that operate in it, are unknown. In either method, and with a polisher of round or oval shape, it is indispensably necessary, that there should be furrows made in the coating of pitch, (to allow it to subside, in regular gradation, from the middle to the edges,) by indenting it, either in squares, as is usually done, or in circular channels; both which must be renewed, as they become filled up and obliterated; which will always happen soonest in the middle zone or tract of the polisher, between the center and other edge, whether the furrows be circular or longitudinal: and, if this be not done, the regularity of curvature would not be preserved in the mirror, or the polisher. But, since there is no obstacle to the subsidence of the pitch, near its outer edge, and its inner edge, when there is a void space at the center, I believe the furrows ought not to be made there, but in the intermediate space only. And I am of opinion that it is, from the judicious disposition of these furrows, the most correct shape of the mirror is to be acquired, whether the polisher be round or oval, or the pitch hard or soft: for I found, that, in Mr. Edwards's method, and with pitch, even as hard as he recommends, the channels made in it were, towards the end of the operation, nearly obliterated, in the middle zone of the polisher. But this will not happen so soon, nor so dangerously, with hard as with soft pitch; nor will the correction of the impaired shape of the polisher be so difficult, when it is of an oval, as when of a circular area: there being, in the former case, less of irregular surface in it, to be reduced; and a more steady, uniform, and simple motion, in grinding, may be pursued; which, as it will admit of a less degree of expertness and sagacity in the artificer, will be more commonly attended with eminent success*.

—but these must be furrows in the pitch.

(To be Continued.)

* I imagine, that a polisher, whose area is of an oval form, would be better adapted to the formation of a parabolic, than an

No. VI.

On the inverted Action of the alburnous Vessels of Trees. By THOMAS ANDREW KNIGHT, Esq. F.R.S. *From the Philosophical Transactions,* 1806.

Theory deduced from facts, by the author: that the sap circulates through the leaves and descends through the bark.

HAVE endeavoured to prove, in several Memoirs* which you have done me the honour to lay before the Royal Society, that the fluid by which the various parts (that are annually added to trees, and herbaceous plants whose organization is similar to that of trees), are generated, has previously circulated through their leaves † either in the same, or preceding

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hyperbolic curvature, in the speculum; and that the latter will be most correctly formed by a polisher, whose area is nearly circular. For, in order to make the speculum hyperbolic, the longest diameter of the oval polisher must be considerably greater than the shortest one, i. e. than the breadth of the mirror: as will be evident, from a consideration of the circumstances I have endeavoured to explain. And, as the mirror must be carried, by the strokes in polishing, to the extreme verge of the polisher; so, when it is to traverse it, in the direction of its longest diameter, it will have its center or vértex removed too far from that of the polisher, to acquire from it a true conoidal figure. Either, therefore, the face of the polisher should be round; or, if it be oval, it ought to be rendered a less eccentric ellipse, by having its shortest diameter greater than that of the mirror, which will allow the extent of the polisher to be reduced, by contracting proportionably its tranverse diameter; i. e. it must be brought nearer to a circular figure. For the objection, mentioned by Mr. Edwards, to a round shape of the polisher, when it is to be considerably larger than the mirror, viz. "that it makes the latter perpetually into a "segment of a larger sphere, and by no means of good figure," I apprehend to have chiefly arisen, from an omission, in those who tried it, to make furrows in the pitch, in the proper tract, on the surface of the polisher; which, if it had been done, would have produced, not a spherical, but a canoidal figure. –

* In the Phil. Trans. for 1801, 1803, 1804, and 1805.

† During the circulation of the sap through the leaves, a transparent fluid is emitted, in the night, from pores situated on their edges; and on evaporating this liquid obtained from very luxuriant plants of the vine, I found a very large residuum

season, and subsequently descended through their bark; and after having repeated every experiment that occurred to me, from which I suspected an unfavourable result, I am not in possession of a single fact which is not perfectly consistent with the theory I have advanced.

Apparent objection from a fact stated by Hales and Du Hamel.

There is, however, one circumstance stated by Hales and Du Hamel, which appears strongly to militate against my hypothesis; and as that circumstance probably induced Hales to deny altogether the existence of circulation in plants, and Du Hamel to speak less decisively in favour of it than he possibly might otherwise have done, I am anxious to reconcile the statements of these great naturalists, (which I acknowledge to be perfectly correct,) with the statements and opinions I have on former occasions commuicated to you.

That the stem below an annular privation of bark, lives and grows, though little.

Both Hales and Du Hamel have proved, that when two circular incisions through the bark, round the stem of a tree, are made at a small distance from each other, and when the bark between these incisions is wholly taken away, that portion of the stem which is below the incisions through the bark continues to live, and in some degree to increase in size, though much more slowly than the parts above the incisions. They have also observed that a small elevated ridge (*bourrelet*) is formed round the lower lip of the wound in the bark, which makes some slight advances to meet the bark and wood projected, in much large quantity, from the opposite, or upper lip of the wound.

Explanation of this fact.

I have endeavoured, in a former Memoir,* to explain the cause why some portion of growth takes place below incisions through the bark, by supposing that a small part of the true sap, descending from the leaves, escapes downwards through the porous substance of the alburnum. Several facts stated by Hales seems favourable to this supposition; and the existence of a power in the alburnum to carry the sap in different directions, is proved in the growth of inverted cuttings of different species of trees.† But I have derived so

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to remain, which was similar in external appearance to carbonate of lime. It must, however, have been evidently a very different substance from the very large portion, which the water held in solution. I do not know that this substance has been analyzed, or noticed by any naturalist.

* Phil. Trans. for 1803. † Ibid. for 1804.

many advantages, both as a gardener and farmer, (particularly in the management of fruit and forest trees,) from the experiments which have been the subject of my former memoirs, that I am confident much public benefit might be derived from an intimate acquaintance with the use and office of the various organs of plants; and thence feel anxious to adduce facts to prove that the conclusions I have drawn are not inconsistent with the facts stated by my great predecessors.

The first motion of the true sap in germination is downwards

It has been acknowledged, I believe, by every naturalist who has written on the subject, (and the fact is indeed too obvious to be controverted,) that the matter which enters into the composition of the radicles of germinating seeds existed previously in their cotyledons; and as the radicles encrease only in length by parts successively added to their apices, or points most distant from their cotyledons, it follows of necessity that the first motion of the true sap, as this period, is downwards. And as no alburnous tubes exist in the radicles of germinating seeds during the earlier periods of their growth, the sap in its descent must either pass through the bark, or the medulla. But the medulla does not apparently contain any vessels calculated to carry the descending sap; whilst the cortical vessels are, during this period, much distended and full of moisture: and as the medulla certainly does not carry any fluid in stems or branches of more than one year old, it can scarcely be suspected that it, at any period, conveys the whole current of the descending sap.

—through the bark.

Cortical vessels from the bases of the leaves, by which the sap descend.

As the leaves grow, and enter on their office, cortical vessels, in every respect apparently similar to those which descended from the cotyledons, are found to descend from the bases of the leaves; and there appears no reason, with which I am acquainted, to suspect that both do not carry a similar fluid, and that the course of this fluid is, in the first instance, always towards the roots.

The ascending sap passes through the alburnum and central vessels.

The ascending sap, on the contrary, rises wholly through the alburnum and central vessels; for the destruction of a portion of the bark, in a circle round the tree, does not immediately in the slightest degree check the growth of its leaves and branches: but the alburnous vessels appear, from the experiments I have related in a former paper,* and from those

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* Phil. Trans. for 1804.

I shall now proceed to relate, to be also capable of an inverted action, when that becomes necessary to preserve the existence of the plant.

Experiments with young oaks.

As soon as the leaves of the oak were nearly full grown in the last spring, I selected in several instances two poles of the same age, and springing from the same roots in a coppice, which had been felled about six years preceding; and making two circular incisions at the distance of three inches from each other through the bark of one of the poles on each stool, I destroyed the bark between the incisions, and thus cut off the stem and roots, through the bark. Much growth, as usual, took place above the space from which the bark had been taken off, and very little below it.

Examining the state of the experiment in the succeeding winter, I found it had not succeeded according to my hopes: for a portion of the alburnum, in almost every instance, was lifeless, and almost dry, to a considerable distance below the space from which the bark had been removed. In one instance the whole of it was, however, perfectly alive; and in this I found the specific gravity of the wood above the decorticated space to be 114, and below it 111; and the wood of the unmutilated pole at the same distance from the ground to be 112, each being weighed as soon as it was detached from the root.

Had the true sap in this instance wholly stagnated above the decorticated space, the specific gravity of the wood there ought to have been, according to the result of former experiments,* comparatively much greater: but I do not wish to draw any conclusion from a single experiment; and indeed I see very considerable difficulty in obtaining any very satisfactory, or decisive facts from any experiments on plants, in this case, in which the same roots and stems collect and convey the sap during the spring and summer, and retain, within themselves, that which is, during the autumn and winter, reserved to form new organs of assimilation in the succeeding spring. In the tuberous-rooted plants, the roots and stems which collect and convey the sap in one season, and those in which it is deposited and reserved for the succeeding season, are perfectly distinct organs; and from one

Experiment with the potatoe.

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* Phil. Trans. for 1805.

of these, the potatoe, I obtained more interesting and decisive results.

My principal object was to prove that a fluid descends from the leaves and stem to form the tuberous roots of this plant; and that this fluid will in part escape down the alburnous substance of the stem when the continuity of the cortical vessels is interrupted: but I had also another object in view.

The early varieties afford neither blossoms nor seeds, because the early tubers consume the true sap which might have formed them.

Every gardener knows that early varieties of the potatoe never afford either blossoms or seeds; and I attributed this peculiarity to privation of nutriment, owing to the tubers being formed preternaturally early, and thence drawing off that portion of the true sap, which in the ordinary course of nature is employed in the formation and nutrition of blossoms and seeds.

Cuttings of the potatoe managed so as to produce no tubers,

I therefore planted, in the last spring, some cuttings of a very early variety of the potatoe, which had never been known to blossom, in garden pots, having heaped the mould as high as I could above the level of the pot, and planted the portion of the root nearly at the top of it. When the plants had grown a few inches high, they were secured to strong sticks, which had been fixed erect in the pots for that purpose, and the mould was then washed away from the base of their stems by a strong current of water. Each plant was now suspended in air, and had no communication with the soil in the pots, except by its fibrous roots, and as these are perfectly distinct organs from the runners which generate and feed the tuberous roots, I could readily prevent the formation of them. Efforts were soon made by every plant to generate runners and tuberous roots; but these were destroyed as soon as they became perceptible. An increased luxuriance of growth now became visible in every plant; numerous blossoms were emitted, and every blossom afforded fruit.

—afforded blossoms and fruits.

The redundant sap was made to afford tubers on the branches instead of the roots,

Conceiving, however, that a small part only of the true sap would be expended in the production of blossoms and seeds, I was anxious to discover what use nature would make of that which remained, and I therefore took effectual means to prevent the formation of tubers on any part of the plants, except the extremities of the lateral branches, those being the points most distant from the earth, in which the tubers are naturally deposited. After an ineffective struggle of a few weeks, the plants became perfectly obedient to my wishes, and formed their tubers precisely in the places I had assigned them. Many of the joints of the plants during the experiment became enlarged and turgid; and I am much inclined to believe, that if I had totally prevented the formation of regular tubers, these joints would have acquired an organization capable of retaining life, and of affording plants in the succeeding spring.

Another experiment.

I had another variety of the potatoe, which grew with great luxuriance, and afforded many lateral branches; and just at that period, when I had ascertained the first commencing formation of the tubers beneath the soil, I nearly detached many of these lateral branches from the principal stems, letting them remain suspended by such a portion only of alburnous and cortical fibres and vessels as were sufficient to preserve life. In this position I conceived that if their leaves and stems contained any unemployed true sap, it could not readily find its way to the tuberous roots, its passage being obstructed by the rupture of the vessels, and by gravitation; and I had soon the pleasure to see that, instead of returning down the principal stem into the ground, it remained and formed small tubers at the base of the leaves of the depending branches.

Hence the tubers are formed by sap descending from the stems or leaves, through the bark.

The preceding facts are, I think, sufficient to prove that the fluid, from which the tuberous root of the potatoe, when growing beneath the soil, derives its component matter, exists previously either in the stems or leaves; and that it subsequently descends into the earth: and as the cortical vessels, during every period of the growth of the tuber, are filled with the true sap of the plant, and as these vessels extend into the runners, which carry nutriment to the tuber, and in other instances evidently convey the true sap downwards, there appears little reason to doubt that through these vessels the tuber is naturally fed.

Growth of the tubers impeded by intercepting the descending sap.

To ascertain, therefore, whether the tubers would continue to be fed when the passage of the true sap down the cortical vessels was interrupted, I removed a portion of bark of the width of five lines, and extending round the stems of several plants of the potatoe, close to the surface of the ground, soon after that period when the tubers were first formed. The plants continued some time in health, and during that period the tubers continued to grow, deriving their nutriment, as I conclude, from the leaves, by an inverted action of the alburnous vessels. The tubers, however, by no means attained their natural size, partly owing to the declining health of the plant, and partly to the stagnation of a portion of the true sap above the decorticated space.

Probability that the sap descends through the alburnum where the plant is decorticated.

The fluid contained in the leaf has not, however, been proved, in any of the preceding experiments, to pass downwards through the decorticated space, and to be subsequently discharged into the bark below it: but I have proved with amputated branches of different species of trees, that the water which their leaves absorb, when immersed in that fluid, will be carried downwards by the alburnum, and conveyed into a portion of bark below the decorticated space; and that the insulated bark will be preserved alive and moist during several days;* and if the moisture absorbed by a leaf can be thus transferred, it appears extremely probable that the true sap will pass through the same channel. This power in the alburnum to carry fluids in different directions probably answers very important purposes in hot climates, where the dews are abundant and the soil very dry; for the moisture the dews afford may thus be conveyed to the extremities of the roots: and Hales has proved that the leaves absorb most when placed in humid air; and that the sap descends, either through the bark or alburnum, during the night.

This inverted action of the alburnum explains the facts in the experiments of Hales and Du Hamel.

If the inverted action of the alburnous vessels in the decorticated space be admitted, it is not difficult to explain the cause why some degree of growth takes place below such decorticated spaces on the stems of trees; and why a small portion of bark and wood is generated on the lower lip of the wound. A considerable portion of the descending true sap certainly stagnates above the wound, and of that which escapes into the bark below it, the greater part is probably carried towards, and into, the roots; where it preserves life, and occasions some degree of growth to take place. But a small portion of that fluid will be carried upwards by capillary attraction, between the bark and the alburnum, exclusive of the immediate action of the latter substance, and the whole of this will stagnate the lower lip of the wound; where I conceive it generates the small portion of wood and bark, which Hales and Du Hamel have described.

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* This experiment does not succeed till the leaf has attained its full growth and maturity, and the alburnum of the annual shoot its perfect organization.

Interesting facts respecting the annular decortication of the fir.

I should scarcely have thought an account of the preceding experiments worth sending to you, but that many of the conclusions I have drawn in my former memoirs appear, at first view, almost incompatible with the facts stated by Hales and Du Hamel, and that I had one fact to communicate relative to the effects produced by the stagnation of the descending sap of resinous trees, which appeared to lead to important consequences. I have in my possession a piece of a fir-tree, from which a portion of bark, extending round its whole stem, had been taken off several years before the tree was felled; and of this portion of wood one part grew above, and the other below, the decorticated space. Conceiving that, according to the theory I am endeavouring to support, the wood above the decorticated space ought to be much heavier than that below it, owing to the stagnation of the descending sap, I ascertained the specific gravity of both kinds, taking a wedge of each, as nearly of the same form as I could obtain, and I found the difference greatly more than I had anticipated, the specific gravity of the wood above the decorticated space being 0.590, and of that below only 0,491: and having steeped pieces of each, which weighed a hundred grains, during twelve hours in water, I found the latter had absorbed 69 grains, and the former only 51.

The wood above the decorticated space is much denser.

Whence considerable advantage may be derived in felling.

The increased solidity of the wood above the decorticated space, in this instance, must, I conceive have arisen from the stagnation of the true sap in its descent from the leaves; and therefore in felling firs, or other resinous trees, considerable advantages may be expected from stripping off a portion of their bark all round their trunks, close to the surface of the ground, about the end of May or beginning of June, in the summer preceding the autumn in which they are to be felled. For much of the resinous matter contained in the roots of these is probably carried up by the ascending sap in the spring, and the return of a large portion of this matter to the roots would probably be prevented:* the timber I have, however, very little doubt would be much improved by standing a second year, and being then felled in the autumn; but some loss would be sustained owing to the slow growth of the trees in the second summer. The alburnum of other trees might probably be rendered more solid and durable by the same process: but the descending sap of these, being of a more fluid consistence than that of the resinous tribe, would escape through the decorticated space into the roots in much larger quantity.

The increased solidity is not confined merely to the vicinity of the decorticated space.

It may be suspected that the increased solidity of the wood in the fir-tree I have described was confined to the part adjacent to the decorticated space; but it has been long known to gardeners, that taking off a portion of bark round the branch of a fruit-tree occasions the production of much blossom on every part of that branch in the succeeding season. The blossom in this case probably owes its existence to a stagnation of the true sap extending to the extremities of the branch above the decorticated space; and it may therefore be expected that the alburnous matter of the trunk and branches of a resinous tree will be rendered more solid by a similar operation.

I send you two specimens of the fir-wood I have described, the one having been taken off above, and the other below, the decorticated space. The bark of the latter kind scarcely exceeded one-tenth of a line in thickness; the cause of which I propose to endeavour to explain in a future communication relative to the reproduction of bark.

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* The roots of trees, though of much less diameter than their trunks and branches, probably contain much more alburnum and bark, because they are wholly without heart wood, and extend to a much greater length than the branches; and thence it may be suspected that when fir-trees are felled, their roots contain at least as much resinous matter, in a fluid moveable state, as their trunks and branches; though not so much as is contained, in a concrete state, in the heart wood of those.

VII.

The Invisible Lady; being an Explanation of the Manner in what the Experiment which was exhibited in London, by M. Charles and others, is performed. In a Letter from a Correspondent.

The experiment of the Invisible Girl has not yet been published.

AS the acoustic experiment of the Invisible Girl, which has excited so much attention and curiosity, does not appear to have been hitherto explained in any publication, I have sent you the following drawing and description of the manner in which it is performed. I must, however, in justice observe, that the conduct of this experiment has been kept in the most profound secrecy by the exhibitors, and that my information was obtained from the account given of it by Mr. Millington, in one of his philosophical lectures, last winter, in Chancery-lane; where I witnessed the experiment in its full effect; and by a comparison of his account with the exhibition which I have since visited, and find perfectly to agree with his description, I am fully convinced they are one and the same thing. If therefore you think the account I send you worthy of insertion in your valuable Journal, it is quite at your service, and it may perhaps afford information, and gratify the curiosity of some of your readers.

It was explained by Mr. Millington in his lectures.

I am, Sir, your's, &c.

X.

The apparatus consists of four metallic trumpets connected with an hollow ball.

Fig. 1, plate 2, represents a perspective view of all the visible apparatus of the Invisible Girl as you enter the room. It consists of a mahogany frame, not very unlike a bedstead, having four upright posts *a a a a*, about five feet high, at the corners, which are united by a cross rail near the top, *b b*, and two or more cross rails near the bottom, to strengthen the frame: these are about four feet in length. The frame thus constructed stands upon the floor, and from each top of the four pillars *a a a a* spring four strong bent brass wires, converging at the top *c*, where they are secured by a crown and other ornaments. From these four wires a hollow copper ball, of a foot in diameter, is suspended by slight ribbons so as to cut off all possible communication with the frame. This globe is supposed to contain the invisible being, as the voice apparently proceeds from the interior of it; and for this purpose it is equipped with the mouths of four trumpets, placed round it in an horizontal direction, and at right angles to each other, as may be more distinctly seen at fig. 2, where *g* is the globe, *d d d d* the trumpets, and *b b b b* the frame surrounding them, being about half an inch from them. When a question is proposed, it is asked from any side of the frame, and spoken into one of the trumpets, and an answer immediately proceeds from all the trumpets, so loud as to be distinctly heard by an ear addressed to any of them, and yet so distant and feeble, that it appears as if coming from a very diminutive being. In this the whole of the experiment consists, and the variations are, that the answer may be returned in several languages, a kiss will be returned, the breath, while speaking, may be felt, and songs are sung either accompanied by the piano forte, &c.

—and the sounds seem to proceed from the trumpets.

After describing the manner in which this effect is brought about, it will immediately appear that the whole deception consists in a very trifling addition to the old and well-known mechanism of the Speaking Bust, which consists of a tube from the mouth of a bust, leading to a confederate in an adjoining room, and another tube to the same place, ending in the ear of the figure. By the last of these, a sound whispered to the ear of the bust is immediately carried to the confederate, who instantly returns an answer by the other tube, ending in the mouth of the figure, who seems to utter it; and the Invisible Girl only differs in this one circumstance, that an artificial echo is produced by means of the trumpets; and thus the sound no longer appears to proceed in its original direction, but it is completely reversed.

But the sound is really conveyed by a tube.

The apparatus necessary to produce this effect, is seen in fig. 3, where *bb* represent two of the legs of the frame, one of which, as well as half the hand-rail, is made into a tube, the end of which opens in the rail immediately opposite the center of the trumpet. This hole is very small, and concealed by reeds or other mouldings, and the other end communicates by a long tin half-inch pipe, *p p*, concealed under the boards of the floor, *f f*, and passing, concealed, up the wall of the room to a large deal case, *k*, almost similar to an inverted funnel, large enough to contain the confederate,

Description of the tube and other apparatus.

and a piano-forte. Any question asked into one of the trumpets, will be immediately reflected back to the orifice of the tube, and distinctly heard by a person in the funnel, and the answer uttered by them, or a song or tune from the piano-forte will be distinctly heard at the mouths of the trumpets, but no where else, and there it will seem to come precisely from the interior of the globe. A small hole closed with glass is left through the funnel and side wall of the room, as at *w*, by means of which the concealed person has an opportunity of observing and commenting upon any circumstance which may take place in the room.

VIII.

Mr. WILLIAM RUSSEL, *of Newman Street, has offered Proposals for publishing, by Subscription, two Engravings of the* MOON IN PLANO. *By the late* JOHN RUSSEL, *Esq. R.A. with the following adress.*

THE late Mr. Russel, celebrated amongst men of science for the production of the Lunar Globe, left, at his death, two Lunar Planispheric Drawings, the result of numberless telescopic observations scrupulously measured by a micrometer: one of which Drawings exhibits the Lunar Disk in a state of direct opposition to the sun, when the eminences and depressions are *undetermined*, and every intricate part, arising from colour, form, or inexplicable causes, is surprisingly developed and exquisitely delineated; and the other, of precisely the same proportion, represents the eminences and depressions of the moon *determined* as to their form with the utmost accuracy, producing their shadows when the sun is only a few degrees above the horizon of each part. The former of these was beautifully and most correctly engraved by Mr. Russell, who had likewise very considerably advanced in the engraving of the latter, when death terminated his labours: it is, however, left in such a forward state, that it will be finished with the greatest exactness, and all possible dispatch.

Mr. William Russell, son of the late Mr. Russell, proposes to publish, by subscription, these lunar plates, which have been long promised to the scientific world: and the first engraving is now offered for their inspection. The whole will be incomparably the most complete lunar work ever offered in any age —a work, the more carefully it is examined, either as to its accuracy or elegance (effected indeed, by *extreme* labour during twenty-one years), the more it will excite the wonder and admiration of the diligent inquirer.

The utility of these engravings is best expressed in the author's own words: "The principal use of the moon to as-
" tronomers, is, that of ascertaining the longitude of places
" by the transit of the earth's shadow, when the moon is eclipsed.
" The shadow of the earth coming in contact with many known
" spots, if the observation be made in different places at the
" same time, the longiaude of each place could by this means
" be ascertained with great precision, provided the spots to be
" made choice of be sufficiently represented and recognised;
" but there being *no* faithful delineation of the moon,
" and the *edges* of those spots which are known being undefi-
" ned, the observations made have not been so useful as
" could be wished: for this purpose, it is believed, Mr.
" Russell's labours will be found very useful, and will very
" much add to the certainty and precision of the observations
" on Lunar elipses; as the chief design of his planisphere,
" representing the moon in a state of opposition to the sun,
" is directed to this end, and which he has spared no pains in
" bringing to perfection."

These engravings, it is expected, will not only prove of great utility to the astronomer, but lead to very important speculations in natural philosophy. The remarkable changes of forms in various eminences, the different radiations of light observable at one age of the moon and not at another, with its numerous surprising phenomena, are in those plates faithfully and fully expressed, so as to form a work, it is presumed, highly interesting in the departments either of Astronomy or Natural Philosophy.

CONDITIONS.

1. The diameter of each Planisphere is fifteen inches.—2. The impressions shall be delivered in the order of subscribing.—3. The price to subscribers for the work is five guineas: an advance will be made to non-subscribers, when the whole has been completed.—4 One half of the above sum to be paid

at the time of subscribing; when one part of the work will also be delivered.—5. The description, &c. of both plates will be given when the second plate is paid for and delivered.—6. The whole of the printing shall be executed by the first printers, upon the best wove paper,

Subscriptions are also received at Mr. FADEN's, Geographer to his Majesty, and to His Royal Highness the Prince of Wales, Charing Cross.

Sir JOSEPH BANKS, K. B. Sir H. ENGLEFIELD, Drs. MASKELYNE, MILNER, HERSCHEL, HON. H. CAVENDISH, Earl of EGREMONT, are amongst the names already subscribed.

VIII.

Letter of Inquiry from a Correspondent, whether the Light and Heat Company is entitled to public Encouragement.

To Mr. NICHOLSON.

SIR,

I SEND you a collection of papers which have been circulated by Mr. Winsor, patentee for lighting apartments by the gas from pit-coal, who is soliciting an immense subscription, in order to establish a public company. On former occasions, I observe that you have not hesitated to give your opinion without reserve, upon subjects by which the public might be benefited. I trust you will suffer the same motives to operate on the present occasion.

Letter of inquiry respecting Mr Winsor's Light and Heat Company.

I am, Sir,

Your obliged and constant reader,

M. P.

REPLY. W. N.

THOUGH I had heard of the scheme in question some time ago, I did not find sufficient motives for paying any particular attention to it, until the papers were sent me by my correspondent, and they now come so late, that I can treat the subject only in a cursory manner. The following remarks will require no apology to my readers for the freedom with which they are made.

The invention was publicly known, long before Mr. Winsor had his patent.

I. To give light by the gas from coal was many years ago done by Lord Dundonald. It was afterwards shewn in public, anno 1784, by Diller and others. Mr. Murdoch (see Philos. Journal, XI. 74. for May 1805) extensively applied this practice in Cornwall in 1792, and afterwards at Soho in 1798, and since. And many years afterwards, Mr. Winsor takes out a patent for this very object. He is not the *first inventor as to the public use and exercise thereof*, and therefore *his patent is void* by the statute of James I.

If Mr. Winsor's patent were for a new invention, he renders it void, by sharing it to more than five.

II. Mr. Winsor, in his paper intitled *Terms and Conditions*, &c. says his patent is vested in four respectable gentlemen, whose names, for the respectability of his project, he ought to have given as well as his own. He proposes, that, *instead of the co-patentees engrossing the whole of their patent privileges to themselves, they will share them with a large and respectable number of their countrymen.* Now as the Letters Patent are merely grants of privileges, it cannot be questioned but that every one who shall be admitted to any share or interest in the privileges so granted, will become a joint patentee; and of course, that if that number exceed five, the patent itself will be rendered void, by virtue of the proviso therein contained for that purpose. The paper here first mentioned is the basis of subscription, and does certainly give an equitable interest in the patent to every one who subscribes, as far as the number five; and after the admission of the sixth subscriber, the patent itself, even if not otherwise exceptionable, becomes a nullity.

There is no probability of his obtaining an act of parliament.

III. Mr. Winsor solicits subscriptions, on the condition that a company shall be established by an act of the legislature, —as if the legislature were at his command, or as if an act of parliament could be had as a thing of course. I do not think it needful to discuss his calculations of profits, and the statistic inferences he pretends to draw from them. Much as they are open to objection, I would only ask, whether the legislature is likely to consent that he and his subscribers shall levy upon the nation an annual interest of 575l. for every 5l. subscription, to the amount of many millions—even supposing his merits as an inventor to be as much above par as my first paragraph appears to place them below it?

IV. Lastly, as a matter of prudence between man and man, I would ask who are the responsible trustees for the subscriptions, which, at five pounds each, would amount to one hundred thousand pounds? I am very far from inviting a discussion of any man's private character unnecessarily, and of Mr. Winsor I know absolutely nothing: but I must say that in a concern of much less apparent magnitude than the present, common sense and common integrity ought to have dictated the insertion of the names of trustees in the printed papers before me. Two respectable banking-houses are indeed named for receiving subscriptions, and I should hope, for their credit as honourable men, that they have consented to be bankers to persons who are known and recommended to them as fair and proper connections, more especially in a project of such apparent moment and doubtful import. I dare not presume the contrary; and all that I have to say on this head is, that it would have been no more than simple justice and open dealing, if they had insisted that the public should also have known where the powers may be placed of drawing for the monies in their hands, and disposing of the whole at the pleasure or discretion of such drawer.

Neither the partners nor trustees of Mr. Winsor are known.

—but they ought to have been declared.

It is hoped that the bankers who know the project, do also know the trustees.

IX.

Observations of Dr. Carradori, showing that Water is not deprived of its Oxygen by boiling.

MESSRS. Humboldt and Gay-Lusac, in an interesting memoir presented to the National Institute, and entitled, "Experiments on Eudiometric Methods, and the proportion of the constitutuent principles of the atmosphere, &c.," are, as it appears, of opinion, that ebullition is the most effectual means of depriving water of oxygen. In effect they have availed themselves of this operation only for obtaining this end; and they afterwards assert, that where water is gradually heated, the proportion of oxygen encreases as the heat approaches to ebullition; whence they have concluded, that in the degree of the heat constituting the temperature of ebullition, the oxygen is most easily driven off, and that there is no other power for disengaging it. But we find, by experience, that ebullition is not sufficient to divest the water of all the oxygen it contains, whether attached or combined. Ebullition deprives water of much of the oxygen and other gas with which it is impregnated, but it cannot entirely separate them; for it is proved that water well boiled always retains oxygen, Nothing but congelation, and the respiration of fishes, can clear water entirely of its oxygen: these two are the only means that complete the separation from water of all oxygen it contains interposed between its globules; for it is not till then that we obtain an exact proof of its being divested of it. As to the rest, the detaching and decomposing power of heat, at the degree of ebullition, is not sufficient to overcome the affinity and attractive power of all the oxygen united with the water; a part of it remains obstinately fixed, in spite of all the heat.

Fishes, as I have elsewhere observed, are the eudiometers of water, and one of those, shut up in a body of water, is capable of separating, by means of its respiration, in several hours, all the oxygen from the water, and to exhaust it entirely of this principle. It is by this method that boiled water is proved to be not entirely divested of oxygen, but still contains it.

If we take a quantity of water, and boil it for any length of time, and then pour it quite boiling into a bottle, or a glass vessel with a narrow neck, so that it be full up to the top; if a portion of oil be poured upon it to prevent the air from penetrating, and it be then suffered to cool: in this state, let the oil be removed, and a little fish thrown in, and the oil be immediately replaced, the fish will continue to live some time in this water, and will be seen to breathe.

Ebullition, therefore, has not removed all the oxygen of the water; but a portion of it remains sufficient to serve for the respiration of the fish; for when the water is really deprived of all its oxygen, fishes thrown into it die instantly, because they cannot breathe. This is a matter of fact that any one can verify.

But again, let us take snow, and introduce it by little and little into a glass bottle, continuing to do so in proportion as it melts and lessens in bulk, till, being entirely melted, it shall fill the bottle with water up to the brim; let oil be immediately poured on the surface, so that it shall rise some inches in the neck of the bottle; let us then permit it to acquire the temperature of the atmosphere, which I suppose to be warm, and capable of melting the snow; if, in this case, by some expedient, the oil placed on the snow water, and which embarrasses the neck of the bottle, be drawn off, and we introduce, with the greatest possible quickness, a fish, as vigorous as possible, and cover the water immediately over with oil, we shall see with what pain this animal is affected in this water; he is attacked with a mortal convulsion, and in a little time ceases to live.

Such water as is obtained from melted snow, is equally obtained from ice or from hail, by introducing pounded ice or hail, by little and little, into a bottle, with the attentions here mentioned; and by throwing in a fish, after these matters are melted, the animal dies in this as in the snow water.

Thus we clearly see that congelation expels from water all the oxygen it contains, and on that account fishes cannot live, because they cannot breathe in it. Water of snow, of ice, or of hail, or water produced from any congealed mass, under whatever form it may be, is a mortal element for the inhabitants of that fluid, because they find it void of oxygen gas, which stops their respiration. These waters are exhausted of oxygen, like that which has served for the respiration of one of these animals until its death.

If a bottle be filled with any kind of water, that is to say, of river, of well, or of spring water, and a little fish be put into it; and afterwards to hinder the water from the absorbing oxygen of the atmosphere, if oil be poured into the neck of the bottle upon the water, the fish will live many hours; but after he shall, by respiration, have exhausted all the oxygen, he will die, Let another similar fish be then thrown into it, he will die as soon as he is in this water. But if it be wished that the water should again become fit for maintaining the life of fishes, this may be instantly effected by pouring it into a large vessel, where it can again absorb the oxygen of the atmosphere. The same observation is true with regard to the snow water; we may render it capable of supporting the lives of fishes, and of serving for their respiration, if we put it in a vessel that shall expose a large surface to the air, in order that it may again absorb the oxygen it has thrown off during the congelation.

Snow or ice water is then, without any doubt, destitute of oxigen, as well as that which has served for the respiration of fishes, who have the faculty, by this process, of separating and of absorbing all the oxygen, which is there in a state of solution.

There is not the slightest difference between snow and ice water, with regard to the privation of their oxygen; both are divested of it. So that what those two respectable physicians have advanced in their memoir, does not appear to be well founded. It appears, according to them, that ice contains a portion of oxygen, but that water in congealing throws off a great part of it, mixed with azotic gas, and that water in its transformation into snow throws off less air than when transformed into ice; because, when they caused snow newly fallen to melt, by gradual warmth, they have obtained from it a mass of air almost double that afforded by melted ice.

It is true we see much air disengage itself from snow while melting: but it is not any air contained in the frozen or chrystalized water which constitutes the snow; but it is an air confined between the interstices of the snow, remaining attached to the faces or surfaces of the chrystals that compose it; and it is for this reason that we see much air proceed from the snow while melting. I have already stated, many of those observations in two memoirs on snow water, inserted in the Journal de Physique of Paris, of Ventose, in the year 7, and of Thermidor, in the year 9; for which reason I shall not extend these reflections to any greater length.

SCIENTIFIC NEWS.

Royal Society.

THE Bakerian Lecture has been lately read before the Royal Society by H. Davy, Esq. F.R.S., on Electricity considered as to its chemical agencies. Mr. Davy has found that a great number of bodies are capable of being decomposed by electricity; particularly those containing alkalies, acids, alkaline earths, and metallic oxides: and he finds that their elements are separated in a voltaic circuit, made with water, in such a way, " that all acid matter arranges itself round the positively electrified metallic point, and all alkaline matter, and the oxides, round the negatively electrified point. In this way, he decomposes insoluble as well as soluble compounds. Sulphuric acid and the earths are separately procured from the earthy neutral compounds, and soda and potash evolved from minerals and stones containing them.

Chemical agency of electricity.

The voltaic current separates acid from their bases; so that the acids arrange themselves round the positive point, and the bases round the negative, &c.

By the attracting and repellent powers of the different electricities, acid and alkaline matter are transported through menstrua for which they have a strong attraction, and through animal and vegetable substances. Thus sulphuric acid will be repelled through an alkaline solution from the negative to positive point, and *vice versa;* potash or lime will be repelled from the positive point to the negative, through an acid solution.

This power is so strong, that it will prevent the usual effects of chemical affinity.

Mr. Davy explains these phenomena by means of some other experiments. As in " Volta's contacts of metals, copper and zinc appear in opposite states; so Mr. Davy finds that acids and alkalies, with regard to each other, and the metals, possess naturally the power of affording electricities," and may be said to be respectively in states of negative and positive electrical energies; and the bodies naturally negative are repelled by negative electricity, and the bodies naturally positive attracted by negatively-electrified points.

Mr. D.'s explanation of these effects is, that he finds the acids and the bases to possess respectively the negative and positive states, and consequently are attracted by the contrary state.

Question: whether chemical affinity and electric energy be not the same power?

As chemical affinity is modified, destroyed, or increased, by modifying, destroying, or increasing the natural electrical states of bodies, and as all bodies that combine chemically, which have been accurately examined as to their electricities, are in states of opposite energy, Mr. Davy puts the question, "Whether chemical unions and decompositions are not the result of the electrical energies of the bodies? and whether elective affinity is not the same property with electrical energy?" He enters into various illustrations and applications of this theory, which naturally arises from the facts.

These general principles explain many galvanic and chemical facts, and afford new methods of analysis.

The general principles explain a number of phænomena before obscure: why acid and alkali were obtained from water apparently pure; the acid and alkaline bases produced by the different poles of the pile; the decomposition of muriate of soda between the plates; the separation of water into oxygen and hydrogen, by attracting and repellent powers acting equally upon other bodies.

The experiments offer new methods of analysis, and will apply to the solution of many natural phænomena.

Printed by Paolo Da Ponte, 15, Poland-Street, Oxford-Street.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1807.

ARTICLE I.

Account of a Fact, not hitherto observed, that the Galvanic Power heats Water while decomposing it in Part. In a Letter from Mr. John Tatum, Jun.

To Mr. Nicholson.

Sir,

Circumstance not hitherto noticed in the galvanic decomposition of water.

In the various galvanic communications which I have had the pleasure of consulting in your invaluable Journal, as well as in lectures and volumes on that interesting subject, to which I have attended, I do not recollect any mention being made on a circumstance attending the decomposition of water, which I observed about two months ago, in preparing for a public lecture I was about to deliver, which, if you think worthy a place in your Journal, is very much at your service.

Experiment. Water was decomposed by troughs of considerable surface.

In the experiment alluded to I made use of four troughs of the following dimensions, viz. two of 26 plates, each plate 50 inches, and two troughs of 25 plates, each plate 36 inches, of course I had the power of 4400 inches. The oxidating fluid I made use of was good nitrous acid, with about sixteen times its bulk of water. On passing the galvanic fluid through about one ounce of water, by means of platina wires, I was much surprised at the quantity of caloric which was liberated as the water became decomposed; the temperature of the tube which contained the water seemed to be about (as near as I could judge by the touch) 180° of Fahrenheit; but as I wished to ascertain the temperature more correctly, I immediately applied the bulb of a thermometer, though it was very unfavourable to ascertain the temperature, as the bulb of a small thermometer could touch a tube of one inch diameter at a very small surface, but the mercury soon rose 10°. Not having any person with me, and endeavouring to regulate the wire which communicated from the battery to the tube of water with one hand, while the other was engaged in holding the thermometer against the tube. I accidentally brought the positive and negative platina wires in contact, which exploded the gases, and forced the tube violently up a considerable height, which falling on the table, broke, and prevented me accomplishing my wish relative to the temperature of the water under decomposition.

Much heat was produced in the water.

The thermometer shewed 10 deg.

Another experiment.

A short time after, in my lecture on the subject, I noticed the circumstance; and one or two of my audience, after the lecture, on applying their hands to the tube while the water was decomposing, hastily withdrew them on account of the heat.

How was the heat produced?

I cannot, Sir, account for the liberated caloric by any other means than supposing that the galvanic fluid furnished more caloric than was necessary to convert the water into gases. If, Sir, this meet with an insertion, I shall communicate more on the above subject the first opportunity; until when permit me to remain,

Your's, most respectfully,

JOHN TATUM, Jun.

Dorset Street,
Jan. 5th, 1807.

II.

Account of the Discovery of the Means of illuminating by the Gas from Coal, by Dr. Clayton, *previous to the Year* 1664. *In a Letter from Mr.* John Webster.

To Mr. Nicholson.

Carbonated hidrogen long ago discovered

I READ with great pleasure your just observations on Mr. Winsor's gaseous proposals for enlightening the inhabitants of this metropolis. The purport of these few lines is merely to say, that the discovery of the carbonated hydrogen gas took place previous to the year 1664.

Happening a short time ago to be reading some of Boyle's manuscripts, in the British Museum, I met with a paper of the following title (Ascough's Manu. 4437).

—by Dr. Clayton.

"Experiments concerning the Spirit of Coals, in a letter to the Hon. Mr. Boyle, by the late Rev. Jas. Clayton, D.D. B. Mus."

As I did not copy the whole of the paper, I have taken the liberty of sending you the few short notes I made at that time.

History of the discovery.

The experiments were undertaken by Dr. Clayton, in consequence of having discovered that gas, issuing from fissures near a coal-pit at Wigan, in Lancashire, ignited when a burning candle was presented to it.

The Doctor distilled coal, and received the gas in a bladder.

Dr. Clayton on observing this effect distilled coal. He first observed that "Fleghm" came over, afterwards a "black oyle," and then an "inflamable spirit." He collected the last product into bladders, and amused his friends by pricking a hole in a full bladder and igniting the gas.

If you think it worth while to insert this in your valuable Journal, it may be the means of gratifying some of your readers by referring them to the original paper, the number of which I have already put down.

From your obliged humble servant,

Jan. 5. 1807. JOHN WEBSTER.

III.

Observations on the Metallic Composition of the Specula of reflecting Telescopes, and the manner of casting them; also a method of communicating to them any particular Conoidal Figure; with an attempt to explain, on scientific Principles, the Grounds of each Process, and occasional Remarks on the Construction of Telescopes. By the Rev. JAMES LITTLE*.

[Concluded from p. 59.]

The polisher must be of pitch, rather soft.

THE consistence of the pitch is, in this business, an article of the first importance. Soft pitch will give to the polish a higher lustre, and will less expose the face of the mirror to scratches: but, if it be too soft, the mirror will sink in it, like a seal in soft wax; and the figure of the polisher cannot be preserved, nor the furrows in it from being defaced. It must, therefore, be always harder than common pitch is, in a mean temperature of the air in this climate. And, after the polishing powder is bedded in it (which must at first be laid on so copiously, that the pitch may not rise up to the surface, between the particles of it,) and when the mirror has been worked on it a little time, then all the loose particles of the powder ought to be washed off, from the edges and furrows of the pitch, with a sponge or brush, (made of fine hair,) under water, that no grains may get on the surface, and injure the polish. And, if this be attended to, and the pitch be a little softened by heat, when the powder is first applied, it may be used of a consistence hard enough, without inconvenience: but, if it be made so hard, as not to sink at all, or expand itself, under the mirror, I believe it will never communicate to it a perfect figure.

The larger the polisher the flatter the figure.

From what has been here laid down, it must be obvious, that, by diminishing the size of the polisher, whether it be of a circular or elliptic shape, the curvature of the mirror will be brought nearer to that of a circle; and, by enlarging the polisher, the curvature will approach to that of an hyperbola.

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* Irish Transactions, Vol. X.

perbola, when the precautions here given are observed. Both these may be done, by spreading the pitch on the polisher, to a greater or lesser extent.

The defects of the large speculum may be compensated by the figure of the smaller.

In the Gregorian telescope, the excess of curvature, in the great mirror, may be remedied, by a defect of it in the little mirror, and *vice versâ*. It must be desirable, to a fabricator of this instrument, to understand why this is so; and how a change in the curvature may be effected: for an artist cannot well execute a project, the design of which is to him unknown; nor improve by trials, even repeated, if they are made in the dark. I apprehend, that, in this kind of telescope, the mirrors are commonly selected, out of a number finished of each size, as they happen to suit each other: and, if there should be but few pairs in the assortment, whose irregularities compensate one another, few good telescopes will be produced. This would be less frequently the case, and the Gregorian telescope be more improved, if a more certain method were known, of giving, to each pair, their appropriate figure at first, or of altering it in either, where it is defective. Perhaps persons, not much versed in optics or geometry, may be assisted, in discovering the evil, and the remedy, from the following remarks; which are given in words, in order to dispense with a diagram.

Popular observations on the conic sections.

The curvature of the circumference of a circle is uniform in every part, being (in an arch of it, of a given length) so much the greater, as the radius is smaller, and *vice versâ*. But the curvature of the ellipse, parabola, and hyperbola, is not uniform, but continually diminishes, from the vertex of these curves, (which answers, in the present case, to the center of the mirror,) to the extremity on each side: but it diminishes less in the ellipsis than the parabola; and in this than in the hyperbola. So that, if we suppose a bow to be bent, at first, into an arch of a circle, and, when gradually relaxed, to become, towards its extremities, more and more straitened, as it unbends, while the curvature, at the very middle, remains the same, it will successively form these three curves, in the above order. And, if concave mirrors had the same curvature with them, they would have the following properties.

The parabolic speculum is good for the

If the speculum be of a parabolic form, rays of light, falling on it, parallel to its axis, or issuing from a luminous point

Newtonian or Gregorian telescope.

point in the same, so very distant, that they may be regarded as parallel, will converge, by reflection, to one point in the axis; which point is the focus. The same is nearly true of rays coming from luminous points not far from the axis, or lying in a very contracted field of view, so as to make but a very small angle with the axis: the rays, coming from each single distinct point in the object, are converged to so many single distinct points in the image, formed at the focus of the mirror. Hence, the excellence of a parabolic mirror, for the larger speculum of the Newtonian or Gregorian telescope*.

If

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* But, because a parabolic mirror reflects, to one point, rays, that fall on it, parallel to its axis, it follows, that it will not converge, to a point, rays, that are diverging or inclined to its axis. The former, (if the point, from whence they radiate, be in the axis of the mirror,) would be reflected from any line, drawn diametrically across the mirror, in a caustic curve double and cuspidated: the latter, (being in the same plane in which is the radiant point, infinitely distant, and the axis,) would form a curve nodated So that the excellence of a parabolic mirror is for viewing remote, but not near objects. And a person might thus be deceived, who would judge of the goodness of a telescope, only from its rendering print legible, at a small distance, from whence the breadth of the great mirror would subtend an angle of sensible magnitude: for the pencils of rays that issue, diverging from each point of the printed letters, will be reflected, by the central part of the mirror, to a focus nearest to it; and the rays of each pencil, that fall on the exterior annuli of the mirror, will be reflected to points more remote from it. So that if, in the Newtonian and Gregorian telescope, the great mirror were of the correct figure of a parabola, and the little mirror, of the latter, were that of an ellipsis; and, if either telescope were adjusted to distinct vision, when the innermost zone only of the great speculum is exposed to the light; yet, the object would be indistinct, if seen by the rays reflected from the outer zone, unless the little mirror were removed farther from the great one. Hence, a spherical mirror is better than a parabolic one, for viewing very near objects; and neither of them can be equally adapted for viewing these and very remote objects. The distinctness of the telescope is, therefore, best proved, by directing it to the stars: if it shews, clearly, the fasciæ, on the disk of the planet Jupiter, or the ring of Saturn, it will deserve to be approved of. I have ground and polished, in the manner here described, the mirrors of a little Gregorian telescope, of nine inches focus, which shewed these objects most distinctly; and I could not afterward, with much greater pains, execute another one, (neither indeed did I ever see one,) of that size, of equal accuracy: which served to convince me, of the exquisite correctness required in

The small speculum ought to be elliptical.

If the mirror be of an elliptic, or oval, curvature, rays, issuing from single points, in, or extremely near to one of its foci, and falling on it, (such as the rays proceeding from the single points in the image, formed by the parabolic larger syeculum,) will be converged to so many single distinct points, in the other focus of the elliptic mirror. Hence, the excellence of an elliptic figure, for the smaller speculum of the Gregorian telescope*. But the case is very different,

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the figure of the mirrors, and of how great perfection the instrument is susceptible. Telescopes have been recommended, for their enabling persons to read gilded letters, at a considerable distance; but this is an improper method for determining their merits; for (beside the ground of error now mentioned) a much greater quantity of light is reflected from gilt letters, than from those of common print on paper.

It is adviseable that the magnifying power of the Gregorian telescope should be chiefly dependent on the eye-piece.

* But this will not be the case, if the rays diverge from points, so remote from the axis of the speculum, as to make a considerable angle with it, and to fall very obliquely on the speculum: which would be the case, if the field of the telescope were too large, or if the focus of the great speculum were too long, with respect to that of the lesser one: because, in either case, the image, formed by the great mirror, which is the object, with regard to the lesser, will have too great latitude; and the extreme pencils, diverging from it, fall, with too much obliquity, on the latter, to be collected by it, to single points, in the second image. And, on this account, there is, in the Gregorian telescope, a limit set to the degree of magnifying, so far as this depends on the mirrors, be their figure ever so correct. And, if any aberration prevail, in the image formed by the larger concave, they will be magnified by the lesser, were it perfect, in the proportion of the focus of the former to that of the latter. I am of opinion, that it is better not to aim at a high degree of magnifying, by the little mirror of this telescope; but, to endeavour to secure the correctness of the second image; and to lay the chief stress of the amplification (as it is in the Newtonian telescope) on the eye glasses; because of the above circumstance, which no correctness, nor compensation of the mirrors, can remedy. From this inconvenience, here stated, the Newtonian telescope (the most perfect of all the constructions, that ever were or ever will be devised) is entirely free. This the author effected, by putting the eye-glasses on a different axis from that of the mirrors; by which he was enabled to make the lesser mirror a plane surface. And it will appear, on due consideration, that he was obliged to introduce this change, in Gregory's telescope, of necessity; and not from a low ambition, to which his mind was superior, that of obtruding his own inventions, to supplant those of equal merit by other men: though he has not stated all the imperfections of the Gregorian form, nor the advantages of his own; having only, in answer to objections, and, as it were, reluctantly, mentioned the chief circumstances, justifying the alteration he had recommended.

in a spherical or hyperbolic mirror. From either of these, the rays, which issue in a cone or pencil, from single, luminous, distinct points, in a very remote object, and fall on them, will not converge again, to so many single points; but will, in the mean focus of the mirror, be dispersed, and blended together in a small degree, yet sufficient to produce an universal haziness and indistinctness, over the whole surface of the object viewed in a telescope, having its large mirror of these forms, because it occurs, with respect to every point in such object; of which the following are the circumstances.

Effects of spherical mirrors.

If the mirror be spherical, those rays, nearly parallel of each pencil, which fall on it, next to its centre, will converge to a point more distant from the mirror, than the focus of any rays, that fall between the centre and outer edge of the mirror. And those, that fall on the outer extremity of it, will converge to a different point, nearest to the mirror: and the rays, which are incident on the several concentrical annuli, indefinitely narrow, of which the face of the mirror is composed, will have an indefinite number of points of convergence; each annulus its own point, and all lying in a series, in the axis of the pencil, between the points, or foci, of the extreme, and of the innermost annulus*. So that no entire incident pencil will, after reflection, converge to one point, unless the radiant point were in the centre of curvature of the mirror.

Contrary aberration of the hyperbolic mirror.

The property of an hyperbolic mirror is of the same nature, but with effects reversed: for, in this, the rays parallel to its axis, which are incident on its outer annulus, will converge to a point the most distant from it; and the rays, falling on its innermost annulus, will have their focus the nearest to it. And this is easy to comprehend: for, as the curvature of the hyperbola continually diminishes from its vertex, on each side, a parallel, or diverging pencil, falling at a distance from the vertex, on a mirror of this form, must (as in the case of a mirror of greater radius, i. e. of less curvature) have its focus formed farther from it, than if it were incident near the middle or vertex, where the curvature of the mirror is that of a circle of lesser radius.

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* This property of a spherical mirror has never, so far as I know, been synthetically demonstrated, by any optic writer, though it is a fundamental theorem in catoptrics. Mr. Robins derisively objected to Dr. Smith, that he had not demonstrated it. The Doctor, I believe, might have retorted the same charge on Mr. Robins. I have some reason to think, it is difficult to give such a demonstration of it, and that it will reflect credit on the person who furnishes it.

These errors are not corrected by the small speculum.

And thus it is evident, that, as the several pencils, reflected by the great mirror, when it is spherical or hyperbolical, do not converge, each to a single point, but to a series of points, whose length is the depth of the focus of the mirror; so, neither do these pencils, in proceeding on to the little mirror, diverge each from a single point, but from the same series of points. So that, though the little mirror were formed truly elliptical, it would not make each of these pencils converge again (at the place of the second image, formed behind the first eye-glass) to single points, but to another series of points; by which the rays of contiguous pencils would be blended with one another, and make the object which is viewed, by means of these pencils, so transmitted to the eye, and, by it, refracted to a third series of points, near the retina, at the bottom of the eye, appear hazy and indistinct.

Inductions.

These remarks will be applied to our present purpose, by considering:

First. That the rays, reflected by the several annuli, in the surface of the great mirror, will fall on the annuli of the same order, in the little mirror; the rays from the outer, inner, or intermediate annuli of the one, proceeding to the like annuli in the other.

Secondly. That the farther the focus, or point of convergence, of any annulus of the great mirror, is distant from that mirror, the nearer will be the point of divergence of this part of the whole pencil, (among the series of such points), to the little mirror. Also, that the interval, between any one point in the series and this mirror, cannot be altered, by moving the mirror, without altering the intervals of all the rest; which, after the telescope is brought to the distinctest vision, cannot be permitted.

Rationale of the figure of the small mirror.

Thirdly. That, if the focus of any annulus, of the great mirror, be farther from it, than those of the other annuli, and, consequently, nearer to the little mirror than those; the rays, issuing from it, to this mirror, will not be reflected by it, to the same point with those of the other annuli, unless the curvature of the corresponding annulus, of the little mirror, be increased, in the proportion of its radius to that of the great mirror; for, then only will the focus of rays, reflected by this annulus of the little mirror, be shortened, as much as, by the effect of that of the same annulus, of the great mirror, it would otherwise be lengthened. The same is true, *vice versâ*, if the focus of any annulus, of the great mirror, be shorter than those of the other annuli.

Fourthly. That, if there be any excess or defect, in the curvature of the great mirror, from that of a parabola, (and, consequently, a contraction, or elongation, of the foci, of the extreme rays of the reflected pencils,) there is no remedy, while this remains, but to make the little mirror so much deficient, or excessive in curvature, from that of an ellipse, (and, consequently, to lengthen or contract the foci of the extreme rays of the pencils reflected by it,) as its focus is, in proportion to the focus of the great mirror; there being no other means of reducing all the rays, of each pencil, to one point, at the second or conjugate focus of the little mirror; by which alone, the second image, consisting of such points, can be formed, and viewed distinctly, through the last eye-glass.

From all which, it is manifest, that, if the curvature of the great mirror be hyperbolical or deficient, then that of the little mirror ought to be spherical or excessive; and if the great mirror be spherical, the other must be parabolical or hyperbolical, according as its focus is long or short, in respect of that of the great mirror.

Instructions for examining the adaptation of the mirrors to each other.

Should the telescope be faulty, from indistinctness of vision, it may be corrected, by altering the figure of either of the mirrors, as shall be most practicable. And, to know what the alteration should be, the method, directed by Mr. Mudge, may be followed, of excluding the light from the central, middle, or extreme zone of the great mirror, by fixing, on the mouth of the tube, three annular diaphragms of pasteboard, &c. answering, in size and shape, to these zones respectively; by removing any of which diaphragms, the light will be admitted to the corresponding part of the mirror. If, then, by help of the adjusting screw, the object be first viewed distinctly, when the inner or central zone, only, of the mirror is uncovered to the light; and it be necessary, afterward, when it is seen by means of the exterior zone only, to remove the little mirror farther from the great one, (by turning back the adjusting screw,) in order to distinct vision; then one, or both of the mirrors, is deficient in curvature, i. e. the great one is hyperbolical, or the small one parabolical. And, on the contrary, if it be necessary, in this process, to bring the little mirror nearer to the great one; then one or both of the mirrors is spherical. For, in the former case, it is plain, that the mean focus of the outer zone of the little mirror is nearer to the second eye-glass, than that of the inner zone; since it is necessary to withdraw that focus, by putting back the little mirror: and the contrary is evident, in the latter case. The former could happen, only by the focus of the extreme rays, of each single pencil, being too far from the great speculum, (i. e. from its being hyperbolical,) and too near to the little one; or from the latter being deficient in curvature, near its edges; and thus throwing the focus of the rays, that fall there, too far from it, and too near to the last eye-glass. The second effect could arise only from a figure of the mirrors, the reverse of this. In the Newtonian telescope, there can be no doubt, where the defect of curvature is, because it has but one concave mirror.

How to correct the curvatures

When it has been thus determined what the defect is, means must be employed to correct it; and it may be expected, that, unless some certain mode, of effecting a different curvature of the great mirror, from that of the little one, is discovered, and skilfully practised, there will be but few good telescopes, of the Gregorian form, constructed. For, if both mirrors be polished, in the same manner and method, it is likely, that the defects in their figure, and the species of their curvature, will be the same in both. Whereas, it has been shewn, that all these ought to be directly contrary in one, from what they are in the other; referring to the parabola and ellipse, as the standard degrees of curvature.

—by means of the polisher, without grinding anew.

Now, the circumstances, which, in the method of polishing above-mentioned, have a tendency to produce particular species of conoids, have been already explained, and need not be repeated. But, as to the means of altering any figure already given, to the great mirror, in the Newtonian telescope, or to either of the mirrors in the Gregorian, which happens to be unsuitable to the other one; I have to observe, that, in my trials, I have found this could be effected on the polisher, without putting the metal to be ground again upon the hones. For if it has, at first, been formed to a tolerably correct figure, of any species, then a very small reduction, of the substance of the metal, will produce a sufficient alteration of its form. If the change required consists in a diminution of curvature, a continuation of the process, under the regulation before-mentioned, will, without any alteration of the polisher, generally, be sufficient to produce it, from the degree of curvature of a circle, to that of the ellipse, parabola, or hyperbola, in order; or from any of these, to the others, in succession*. But, if the degree of curvature, already given, is to be increased, and to verge more toward the circle, as the limit, (beyond which no contrivance could carry it,) then the polisher must undergo an alteration. Its breadth should be diminished; the space, at the centre, left uncoated with pitch, should be greatly contracted; and, in the case of the little mirror, which has no perforation in it, entirely filled up; save that a small hole, through the polisher, tapering, from the back of it, upwards, to its surface, should be left, for the pitch to sink into, when it becomes closed, and too much compressed, at the centre; and the furrows, in the pitch, gradually deepened toward the edges. I believe, that (for the reason before given) the uncoated space, at the center, ought always to be as much smaller, on every side, than the perforation in the mirror, as the greatest range of the strokes, in polishing, advances the centre of the mirror beyond that of the polisher, having the same shape as the polisher itself; and that it ought to be smallest, or no other than as just mentioned, when there is no hole in the mirror.

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* Here it may be proper to observe, that, as the curvature is constantly diminishing by the mere continuance of the operation, so the process is not to be pursued any longer, after the polish, and the desired figure, are found to be perfected. And the metal must always be brought to a very fine face, and a correct spherical figure, on the hones, or on a leaden tool, bedded with the finest washed emery, before the process of polishing commences; because if all scratches, from the grinding, be not previously obliterated, the polishing must, in order to efface them, be continued so long, as to diminish the curvature of the mirror beyond what is requisite; especially, if the area of the polisher be not of an oval, but of a round shape; which latter has a greater tendency, than the former, to diminish the curvature of the mirror and to render it hyperbolical. And the correction of this, afterward, might require a troublesome alteration of the polisher, or even make it necessary to put the metal again upon the hones; and yet, in the Gregorian telescope, the hyperbolic figure is the proper one, for either of the mirrors, if that of the other speculum be spherical.

Experience confirms this method.

I have, in this method, with certainty of success, as verified by examination of the progressive change of curvature in the mirrors, from a greater degree to a less, and *vice versâ*, effected the desired configuration of them: which serves to confirm me in the belief, that the circumstances, above proposed, are those which are really operative, in communicating the diversity of figure to telescopic mirrors; and that neither the direction of the strokes in polishing, the size or form of the polisher, consistence of the pitch made use of, or other accidents, are of any farther moment in the process, than as they serve to modify the resistance of the pitch, in the several parts of the surface of the polisher. Whether, by attention to the principles here laid down, it would be possible to produce an hyperbolic form, in the convex mirror of the *Cassegrain* telescope, I have been prevented from endeavouring to ascertain by experiment, from those casualties, affecting my situation in life, which I have already intimated. But it should seem, that it would, to a certain degree, be practicable, from the means I have suggested, of producing a progressive, specific alteration in the figure of the polisher; if I have judged rightly of the existence and cause of that alteration.

And if it should be found possible to give, to a convex speculum, an hyperbolic curvature, the same could be done to the convex object glasses of dioptrical telescopes: which is a property still wanting to them; a want which makes them inferior to reflectors, even when they have been rendered achromatic, if the aberrations from their spherical figure remain, after those from refrangibility are removed: which aberrations, taken laterally, as they always ought to be, are as the cubes of the apertures. So that, if the lineal aperture be doubled, and the light admitted, which is as the square of the aperture, quadrupled, in order to increase the magnifying

Proposed improvement of refracting telescopes.

magnifying power four times, (or to double the lineal amplification,) preserving an equal brightness in the image; the telescope must be made four times longer, that it may remain equally distinct *; an inconvenience, from which it must be very desirable to exempt this telescope, by correcting the figure, and, with it, the aberrations of its object glasses.

Observation of Huygens that very small pencils produce indistinct vision.

It is not the object of this essay, to investigate any particulars, in the construction of the telescope; (which would open a large field of inquiry;) but to try to assist mechanical contrivance in its fabrication. To this end, I think it fit to add a remark, on the property of the pencils of rays, respecting the latitude of each of them, where they fall on the pupil of the eye; first discovered by the great Mr. Huygens; as I suspect that the inconvenience he mentions, as resulting from a certain breadth of the pencil, may casually exceed the limit stated by him, and may admit of a practical remedy.

The author thinks that the indistinctness arises from faults in the object-glass, and not in the eye itself;

He observes, that if the latitude or breadth of the pencils, at the pupil of the eye, be very small, (so as not to exceed, if I remember right, the sixtieth part of an inch,) the vision, by the telescope or microscope, will be indistinct; so that, unless the pencils be of greater breadth than this space, at the place of the eye, the instrument will be defective: and he attributes this to something in the natural conformation, or in the humours of the eye; which, consequently, will admit of no remedy. On this may I presume

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* A conception of this may be, perhaps, most familiarly acquired, by considering, that if, of two object lenses, the aperture, and also the focus of one, be twice as great as those of the other, the angles of incidence, and refraction, of the extreme rays, which come from a very remote object, and form the cones or pencils made by both, will be equal; and the pencils themselves, and their aberrations, will be similar figures; all the lineal measures of that of the larger lens, being double those of the other. In order, therefore, to reduce the lateral error or diameter of the circle of aberrations of the former, to an equality with that of the latter, while the aperture, which is the base of the pencil, remains double; this pencil must be made twice more narrow or acute than before. And, to effect this, its length or focus (which, at first, was twice as great as that of the other pencil) must be doubled; so that now it must be four times as long as the smaller pencil; i. e. the lengths should be as the squares of the apertures.

to observe, that the latitude of the pencil, as it enters the eye, is the same as that with which it falls on the last eye-glass; and, that the effect of it, which Mr. Huygens attributes to the eye, may, therefore, as naturally, be attributed to the eye-glass, as to the eye*; especially when an anatomical dissection of it will demonstrate, that the perfect transparency of its humours, and exquisite polish of its cornea, or outer coat, (not to talk of its achromatic property,) far exceed, in this optic instrument of the Deity, any thing that can be manufactured by man. In fact, the polish given to the eye-glass, and the transparency of the glass itself, is always imperfect; and many points in its surface, which ought to serve for the regular transmission of light, will be obstructed by its roughness and opacity; so that, if the pencils occupy but a very little space on the lens, no points of fair transmission may there remain; and the few rays, that pass through, may be so distorted, by irregular refraction, and inflection, in the glass, and in the eye-hole, that the vision must be indistinct. And this was the more likely to happen, in Huygens's time; because, neither the fine polishing powder, of colcothar of vitriol, was then in use; (and Mr. Huygens used nothing but tripoli;) nor was the method of polishing, by the help of pitch, divulged by Sir Isaac Newton. If this conjecture be right, the remedy is, to use both these helps, in communicating an exquisite polish to the eye-glasses; especially the smaller one, where the breadth of the pencils is reduced, in the same proportion as its radius, or as the increase of its magnifying power; and, also, to avoid using flint glass for this purpose; as it is found to be the least transparent of any, as well as most dispersive.

and therefore proposes that these should be improved as to their materials and polish.

I may here also observe, that, as the transparency and polish of glass must ever be, to a certain degree, imperfect; so the projected improvement of Mr. Ramsden, to avoid the dispersion of the rays, by throwing the image just before the first eye-glass, is unlikely to answer: because, in this case, each of the pencils would occupy little more than

Hence Ramsden's eye-piece is objected to.

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* When a white ground is viewed through a telescope which gives a very slender pencil of rays, the want of transparency in the parts of the eye is seen by spots and other figures, which move along with that organ while the telescope is kept motionless.—N.

a point, on the surface of the lens; and, if that point should be opaque, or unpolished, or covered with dust, no rays of the whole pencil could be transmitted through it: which would, probably, happen to too many of the pencils, and affect the image, which is composed of these pencils; whose latitude, therefore, ought to be greater than the limiting measure stated by Mr. Huygens. And, to effect this, with as high a charge as the instrument will bear, a part of the amplification should be thrown away, on the first eye-glass; and diminished, by shortening its focus; that the pencils being, by it, rendered more obtuse, may fall, with greater divergence, and latitude, on the second eye-glass, which may thus be made shorter, and on the eye. For it is not to be supposed, that the image, formed in a telescope, can be viewed, by a small lens, with as much advantage as an object may. The rays, from each point of the latter, fall upon the whole surface of the lens; and, therefore, a sufficient number of them, to fill the pupil of the eye, must be transmitted; whereas, the rays, from each point of the image, occupy only a small speck on the lens; in no case larger, in effect, than the pupil of the eye; and must, when so contracted, be the more obstructed, by any imperfections in the glass.

The common colcothar objected to.

As, therefore, the fineness of the powder, employed in giving the highest lustre and polish to the specula, and to the eye-glasses, is of great importance, in that process; and, as I found, that the crocus, or colcothar of green vitriol, (now used, as the fittest for that end,) could seldom be procured, so free from grittiness, as to be capable of levigation, to a sufficient degree of fineness; insomuch, that I was obliged to attempt to make it myself; it may be useful to state, that, by the following easy method, I succeeded in producing some of excellent quality.

The author makes his colcothar by slowly roasting the copperas, or green sulphate of iron.

Considering that the vitriol, distilled in close vessels, might probably contract this grittiness, in its calx, from an union of some of its component parts, or principles, with the water contained in the vitriol, (which is the metallic salt of iron,) and that this might prevent its perfect calcination; I thought it best to perform the distillation in the open air, and to begin with exhaling the water. Accordingly, I commenced with slowly roasting the vitriol or copperas, broken into grains as small as shot, by holding it over the fire, on a fire-shovel, till the moisture in it appeared to be dried away. I then put it into a crucible, and kept it uncovered, in a clear fire, till it had been some time red hot*; by which, the spirit or oil of the vitriol was distilled from it, and the calx or colcothar remained, of a brownish red colour, and of a perfectly equal texture, entirely free from the hard or gritty particles; and it was easily levigated, when moistened with water, on a piece of looking-glass-plate, by a piece of the like glass having a handle of brass cemented to it. This furnished a very fine and impalpable powder, capable of communicating to the specula, or lenses, the most exquisite polish and lustre†.

And levigating the oxide with water.

Good specula may form a bad telescope if either the specula or the lenses be badly centered, or not placed on the same axis.

To apply with precision, and afford a fair trial of the method of polishing I have recommended, it is necessary farther to consider, that the advantages, resulting from correctness of figure, in the mirrors, may be frustrated, by an undue position of them, or of the lenses, in the instrument, or, by a defect of form in the lenses, whose edges may happen to be thicker on one side than on the other; i. e. they may not be complete, or equally curtate segments of spheres; and, consequently, that a proper trial and estimate cannot be made, of the figure of the mirrors, unless these and the eye-glasses be right in these respects; especially in the Gregorian telescope, whose adjustment is a matter of more nicety and difficulty, than that of the Newtonian. And since, in the former, the surfaces of the mirrors and lenses ought all to have one and the same axis, viz. that of the instrument, in which are to be all their foci; it is necessary this should be cautiously ascertained; because contrary deviations of them, in this respect, might apparently compensate one another, and escape detection, though they would really be attended with the aberrations of enlarged apertures.

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* I suppose that the fire ought not to be too high, or too long continued in this process, lest it should convert the calx of the iron into glassy scoria. Experiments will determine the due regulation of the heat, so as to ensure success to the operation in every instance. The heat ought to be so great, as to give the colcothar, not a brown, but a red colour.

† The same powder, spread on leather, would give the smoothest edge to razors and lancets, &c

Directions for placing the mirrors.

The centre of the great mirror ought to be in the common axis of the instrument; and the position of it in its cell, in the tube, may be known thus. Let the little mirror be taken out of the tube; and let the round, central diaphragm, before-mentioned, (which ought to be made of a flat piece of tinned plate, or a brass plate, made clean and bright enough, to reflect the light strongly, but not polished,) be fastened across the mouth of the tube, exactly in the middle of it; and let a round hole be made through the plate, the centre of which shall be in the axis of the tube, and its diameter so large, as that the whole disc of the sun may be viewed through it, at the eye-hole of the telescope, when the eye-glasses are taken out. Then, directing the instrument to the sun, or the full moon, when very bright, so as that its whole disc shall be seen through the hole in the diaphragm; (using a lightly tinged screen-glass, to look at the sun;) if the light, reflected from the great mirror to the diaphragm, occupies on it, a circular area, concentrical with the hole made at its centre, the mirror is rightly placed, and its focus is in the axis of the tube. But, if the edge of the illuminated circle approaches nearer to the hole in the plate, on any side, the same side of the mirror inclines toward the axis of the tube, its cell not being exactly vertical to it; or otherwise, the centre of the mirror is not in that axis, as it ought to have been. If the outer one, of the three aforesaid diaphragms, be, at the same time, applied to the mouth of the tube, so as to expose only the middle zone of the great mirror to the light; the circle of light, reflected by it, to the central diaphragm, will appear better defined on it.

The speculums may be very truly set by the solar rays,

But the adjustment of both mirrors and lenses may, at the same time, be proved, by the following most easy and certain method, if exactly pursued:

Having provided, that the little mirror shall be so supported, that its centre may always move in the axis of the great tube; and proved that it is so, as Mr. Edwards prescribes, by taking off the mirror, and seeing, through the eye-tube of the telescope, (without the lenses,) that the hole, in the middle of the little round plate, to which hat mirror is screwed, concentrical with the plate, corresp. nds with the intersection of two cross hairs, tied diametrically across the mouth of the tube: then let the little mirror be replaced, and the eye-tube taken out, and let the telescope be directed to the sun's centre; which may be done, by the help of the little dioptrical telescope, called *a finder*, affixed to the instrument, if it be furnished with such; or otherwise, may be effected, with sufficient exactness for this purpose, by pointing the telescope to the sun, so as that the shadow of the little mirror may coincide with the hole, in the great mirror: which will be, when the great tube is so placed, as to project its shadow of a circular form. In these circumstances, let the light, reflected from the little mirror, through the round perforation in the great one, be received upon a plane, placed at some distance behind the latter, at right angles to the axis of the tube. The light will occupy a circular area on the plane; and, if the centre of the light be coincident with that of the shadow of the little mirror, this mirror is not only parallel to the great one, but both are duly adjusted, at right angles, to the axis of the tube; which, also, then corresponds with their axes. But, because the little mirror and its shadow, and also the cone of light, reflected from this mirror, are of greater breadth than the perforation in the great one; the boundaries of the reflected light, and those of the shadow, cannot be seen wholly on the plane, through the hole in the great mirror, in any one position of the telescope. Let, therefore, the axis of the telescope be a little diverted from the centre of the sun till the shadow of the edge of the little mirror falls within the hole in the great one: by which, some direct light will pass through, next the shadow, and appear on the plane, in form of a crescent: and, at the same time, the circle of the reflected light of the sun will have moved across the shadow; till, by a certain degree of obliquity, in the direction of the telescope, the edge of the circle of the reflected light will be in contact, externally, with the crescent of the direct light. And, if the crescent be always of the same breadth, when this contact takes place, on every side, by a diverting of the telescope, from the centre of the sun, successively, in every direction; then both the mirrors are parallel, and at right angles, to the axis of the instrument. But, if the crescent be broader, in any certain position of the tube, when the circle of reflected light just touches its edge; then the side of the little mirror, corresponding with that of the illuminated circle, where it is in contact with the crescent, makes too great an angle, with the axis of the instrument; and it must be reduced to a right angle, by screwing forward the adjusting screw of the little mirror, in that part, having previously withdrawn the opposite screws. When the mirrors are thus found to be rightly placed, and the eye-tube and lenses are restored to their places; if the whole image, of the great mirror, be not visible in the eye-hole, when this is in the common axis of the instrument, then the lenses are defective; either, some of them, or some of their surfaces, or the tube they are fixed in, being inclined to the common axis; and, by this means, distorting the cone of rays, from it. Which irregularities must be rectified, before a true estimate can be formed, of the correctness of the mirror's curvature.

— and the eye lenses afterwards set.

Position of the eye-hole.

The distance from the smaller lens, at which is the point of decussation of all the pencils of light, and the place where the eye-hole ought to be, may also be most easily and most accurately found, by directing the telescope to the sun, having taken off that part of the eye-tube behind the lenses; and letting the light fall on a plane, moveable back and forward, behind the second eye-glass, and kept at right angles, to the axis of the specula and lenses; the place, at which the image of the great mirror, with the shadow of the little mirror described on it, is seen most distinctly formed on the plane, ought to be the place of the eye-hole.

IV.

On the Absorption of Electric Light by different Bodies, and some of their Habitudes with respect to Electricity. In a Letter from Mr. WM. SKRIMSHIRE, Jun. *Communicated by Mr.* CUTHBERTSON.

Wisbech, Jan. 14, 1807.

DEAR SIR,

Phosphorescence of bodies by electricity.

AS you thought my former account of some electrical experiments on the phosphorescency of calcareous substances worth sending to Mr. Nicholson's respectable and valuable Journal, I now trouble you with a second letter, containing the results of other experiments, made with the same view, upon the different species of the remaining earths that were within my reach. But I do not intend to stop here.—And as I perceive the chief value of my inquiries will rest upon their being a dépôt of numerous facts, whereon other philosophers may more successfully build than my abilities will enable me to do, it becomes necessary for me in this, and any future communication upon the same subject, to speak more in detail than I at first intended. If in this state, you should still think them worth public attention, I shall be glad to see them inserted in the Philosophical Journal.

Barytic Genus.

Barytic compounds.

Carbonate of barytes afforded no spark, but was very luminous when the shock was passed above it, though the light was of short continuance.

Sulphate of barytes.—The several specimens of this substance, which were subjected to experiment, were all luminous by the electric shock, but were not so brilliant as the carbonates; they also differed from them in giving very good sparks when placed upon the prime conductor; except some specimens, consisting of small crystals, which acting as so many points, gave out the electric fluid to the atmosphere, and would not allow a spark to be taken from them. It is curious, and worthy observation, that in these two barytic species the facts turn out exactly the reverse of what takes place in the calcareous genus, in which the carbonates give sparks, though they are slightly luminous, compared to the sulphates, which are brilliantly phosphoric, but afford no spark; whereas in the barytic genus the carbonates are beautifully luminous, but give no sparks, while the sulphates afford good sparks, but are slightly phosphorescent.

Sulphuret of barytes.

Sulphuret of barytes was but slightly luminous by the electric explosion; in which it essentially differs from the sulphuret of lime*, which is the most brilliant phosphorus, both by the electric and solar light, that I have yet seen.

Muriatic Genus.

Muriatic genus.

Magnesia, pure, and carbonated, were both luminous by the electric explosion; the light, however, continued but for a short time.

Sulphate of Magnesia is very luminous through its whole substance.

Sulphuret of Magnesia is luminous, but not more so than the carbonate.

Turkey tobacco-pipes.—The bowls of these articles afforded tolerable sparks, but were scarcely luminous, except in the track of the electric fluid, when the points of the discharging rods rested *upon* the surface.

Chlorite gave sparks, which, upon its surface, branched off in minute, different-coloured points, something similar to, though not so brilliant as, the spark taken from any lacquered substance, such as gilt leather, or lacquered wooden ornaments. The explosion rendered it luminous.

Steatites, Talc, and fibrous Amianthus, gave sparks, and were slightly luminous by the explosion.

Asbestus.—A thin polished slab of this stone gave sparks similar to Chlorite, but the ramifications upon its surface were more numerous, and more variegated.

Mica gives sparks.

Mica affords sparks, but I could not observe it luminous by the explosion. When held in the hand it allows the sparks to run along its surface, to strike the finger at a considerable distance, and to which it gives the sensation of a shock, rather than that of a spark. When the shock is passed along its surface, the fluid leaves an indelible mark, similar to its effect upon glass.

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* Canton's phosphorus is so readily illuminated by electricity, that a large lump, newly made, partaking of the exact shape of the crucible, and having never been exposed to light, being placed upon the prime conductor, was beautifully bespangled with brilliant spots, merely by taking the spark from various parts of its surface.

Micaceous Schistus gave a spark, which was ramified upon the surface of the stone, and somewhat more coloured than in the Chlorite. It was scarcely phosphorescent, except in the track of the electric fluid.

Argillaceous Genus.

Argillaceous genus.

Alum affords a purple spark, which is rather ramified upon its surface. It is luminous through its whole substance, when the explosion is made *above* its surface; but it is shattered to pieces when the shock is passed through it.

Pipe-clay gave a spark, and was luminous by the explosion; but when it was formed into tobacco-pipes, and baked, it was scarcely if at all luminous, though it continued to afford sparks.

A greenish, foliated clay, found near the surface of the ground at Derby, gave a tolerably good spark, and was very phosphoric by the explosion taken *above* it. The luminous track left by the electric fluid, when the points of the dischargers rested *upon* the surface, continued several minutes.

A blueish clay*, dug at Wisbech, and provincially termed Gault, or Golt, affords a spark, and becomes luminous by the explosion.

Slate clay, or Shale, affords a spark, and is luminous; but from trials made with different specimens, it appears to lose its power of absorbing light in proportion as it becomes bituminous †.

Slates.—All the slates afforded sparks, and absorbed

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* This clay, which is frequently dug in the Fens, near Wisbech, contains sulphur, and if, when fresh dug, it be held before a fire, it gives out a strong suffocating odour of sulphurous acid.

† At the time of writing the above observation on bituminous shale I had not the most distant recollection of a paper on the light of natural phosphori, by M. Carradori, translated in an early number of the Philosophical Journal, where it is mentioned, that luminous rotten wood, and other such like substances, "become phosphorescent in proportion as they have lost their inflammable principle, and that the property of absorbing, and retaining the light, depends on that circumstance."—Nicholson's Journal, 4to series, Vol. II p. 135.

electric

Argillaceous genus.

electric light from the explosion. But a slate used in this neighbourhood, brought from Colly Weston, in Northamptonshire, and which effervesces with acids, is by far the most beautiful. When the shock is taken *above* the centre of a piece some inches square, not only the part immediately below the rods is luminous, but the surface of the slate appears bespangled with very minute brilliant points to some distance from its centre; and when the points of the dischargers rest upon the surface of the slate, these minute spangles are detached, and scattered about the table in a luminous state. The track of light between the rods continues phosphorescent several minutes.

Hone stone, of a dirty greyish colour, gave a good spark, and was phosphoric by the explosion.

Fuller's Earth gave a good bright spark, but was very slightly luminous, except in the track the fluid left in its passage from one rod to the other.

Reddle gave no spark, but a purple stream, attended with a very sharp hissing sound. It was rather more luminous than Fuller's Earth.

Armenian Bole affords a spark, which is ramified upon its surface. It is not luminous by the electric explosion; even when the points of the dischargers rest upon it, no track of light is visible; but several minute fragments are shivered from its surface.

Terra Sigillitica of the shops gives a spark, and is phosphoric after the explosion.

Basalt gives sparks which are radiated upon its surface, but not ramified as in Chlorite and micaceous Schist. It was phosphoric only in the track formed by resting the dischargers upon it.

Bricks of various kinds afforded small purple sparks, of a bright red colour on the surface. They were very slightly luminous by the explosion; but afforded a bright track of light between the points of the dischargers when they rested upon them.

Tiles of different kinds afforded similar results to the bricks, except that most of the tiles were rather more luminous than the bricks, when the electric explosion was made above them, especially a yellow tile, with a redish tinge in the fracture, which, from its greater phosphorescence,

cence, and its slightly effervescing with acids, I suspect to contain more calcareous earth in its composition than the others.

Queen's ware, glazed, gives a good spark, which is flame coloured, and radiated on its surface; but it is not phosphoric. When fractured, the unglazed surface affords a purple spark, and is luminous by the shock.

All the different kinds of Pottery-ware which I tried gave the same results, except slight varieties in the colour of the sparks: and a common dirty white ware, which was luminous on its glazed surface when the shock was passed above it. From what I have hitherto observed, I am induced to believe that all glazing, in which a metallic oxide is used, is not phosphoric, but gives a good spark.

All unglazed Pottery is luminous by the explosion, and gives a vivid track of light when the dischargers rest upon its surface.

Siliceous Genus.

Rock Crystals were all phosphoric by the explosion; and some of them that had two or three particles of ore upon their surfaces, were transparent by the spark when it passed from the ore to the knob of the discharging rod, otherwise the crystals gave no sparks, but merely a hissing stream. A rhombic crystal was rendered luminous through its whole substance by the explosion, retaining its light several minutes. At the instant after the explosion it emitted a red light, but which very soon became white.

Siliceous genus.

Siliceous sand, washed and dried, was not luminous, except where the points of the dischargers were in contact with it.

Quartz is phosphoric, and shines with a uniform dull white light. It gives a purple stream instead of a spark. After the explosion it affords the same odour as when two pieces are rubbed together.

Flints afford small purple sparks, both from the external coat and the surface of the fracture. The explosion does not render them so luminous as Quartz, but the external coat is more phosphorescent than the fracture, especially in the track of the discharge.

Siliceous genus.

Lapis Lazuli affords a very good spark, and is luminous by the shock.

Egyptian Pebbles, Scotch Pebbles, Felspars, Agates, Calcedonies, Carnelians, and Jaspers, gave hissing purple sparks, and were luminous by the explosion. Several of these substances give out the same odour as when two pieces are rubbed against each other.

Porphyries and Granites gave a hissing purple spark, and were luminous by the shock, which, when passed upon the surface, produced a very bright track of light, which in some specimens, especially in a piece of whitish Granite, continued luminous for several minutes.

Pudding Stones gave a similar hissing spark; and the oval pebbles being more luminous than the siliceous sand, in which they are imbedded, were readily distinguished in the dark when the shock was passed above the surface of the stone.

Mochoas gave very good sparks from the arborescent parts, but only a hissing stream from the stone itself, which is slightly luminous by the shock, but affords a bright track of light between the ends of the discharging rods.

The Yorkshire Stone, which is used for flat pavement, gives a purple spark, and seems to become luminous by the electric explosion, in proportion as it partakes of a calcareous nature, for those specimens which are verging toward micaceous schist, (and in which I have found lamina of sulphur nearly the tenth of an inch thick,) are scarcely at all luminous.

Pumice Stone on some parts of its surface gave only a hissing stream, but on others very good sparks, which appeared to penetrate through its substance, as if it contained some metallic particles within it. The shock rendered it slightly luminous, but it afforded a very bright track of light along its surface, between the ends of the dischargers.

The semivitrified ashes of a Haystack, which was consumed by spontaneous combustion, gave only a hissing stream, and was slightly luminous by the electric explosion; but when the shock was passed upon its surface it afforded a bright track of light.

Various kinds of Glass are not luminous, neither do they give a spark. But the dark green glass of which wine bottles are made, when by exposure to air and moisture, or under other circumstances which may enable it to reflect the prismatic colours with brilliancy, is capable of giving a spark, and emitting light, after the electric explosion has been made above its surface.

Strontian Genus.

Strontian genus.

Native Carbonate of Strontites, instead of a spark, gave only a hissing purple stream, but was very luminous by the explosion.

I remain,
Your's, &c.
W. SKRIMSHIRE, Jun.

V.

Observations upon the Marine Barometer, made during the Examination of the Coasts of New Holland and New South Wales, in the Years 1801, 1802, *and* 1803. *By* Matthew Flinders, *Esq. Commander of his Majesty's Ship Investigator. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S. &c. &c. &c. From Philosophical Transactions for* 1806.

Isle of France, Aug. 19, 1805.

Great advantages of a fore-knowledge of the weather. The barometer indicates it.

A FORE-KNOWLEDGE of the wind and weather is an object so very interesting to all classes of men, and the changes in the mercurial barometer affording the means which appear most conducive to it, a system that should with certainty explain the connection between the variations of the mercury and those in the atmosphere under all circumstances, becomes greatly desirable; to seamen, more especially, whose safety and success depend so much upon being duly prepared for changes of wind, and the approaching storm, it would be an acquisition of the first importance: in a more extended view, I may say, that the patriot and the philanthropist must join with the philosopher and the mariner in desiring its completion. So long and widely-extended a course of observation, however, seems requisite to form even a basis for it, that a complete system is rather the object of anxious hope than of reasonable expectation. Much has been done toward it, but so much appears to remain, that any addition to the common stock, however small, or though devoid of philosophical accuracy, I have thought would be received by the learned with candour. With this prepossession, I venture to submit to them some observations upon the movement and state of the mercury upon the coasts of New Holland and New South Wales, the Terra Australis, or Australia, of the earlier charts.

The land and sea winds are more particularly indicated, and also their strength.

The principal circumstance that has led me to think these observations worth some attention, is the coincidence that took place between the rising and falling of the mercury, and the setting in of winds that blew from the sea and from off the land, to which there seemed to be at least as much reference, as to the strength of the wind or state of the atmosphere; a circumstance that I do not know to have been before noticed. The immediate relation of the most material of these facts, it is probable, will be more acceptable than any prefatory hypothesis of mine; and to it, therefore, I proceed; only premising, that a reference to the chart of Australia will be necessary to the proper understanding of some of the examples.

My examination of the shores of this extensive country began at Cape Leuwen, and continued eastward along the south coast. In King George's Sound, December 20, 1801, after a gale from WSW, the mercury had risen from 29,42 to 29,84, and was nearly stationary for two days, the wind being then moderate at NW, with cloudy weather. On the 22d, the wind shifted to SW, blew fresh, and heavy showers of rain occasionally fell; but the mercury rose to 30,02, and remained at that height for thirty hours; and on the weather clearing up, and the wind becoming moderate in *the same quarter*, it rose to 30,28.

Fresh breezes from E and SE caused a rise in the barometer in King George's Sound, once to 30,20, and a second time to 30,18, although the weather at these times was hazy: but with light winds from the same direction, which were probably local sea breezes only, the mercury stood about 29,95 in that neighbourhood.

2d Example. Jan. 12, 1802, in D'Entrecasteaux's Archipelago,

pelago, the mercury rose to 30,23, previously to a fresh breeze setting in from the eastward. In the evening of the 13th it blew strong from ESE, with hazy weather, and a rapid fall of the mercury to 29,94 had then taken place; but instead of the wind increasing, or bad weather coming on, the wind died away suddenly, and a light breeze came off the land at midnight, with cloudy weather.

Observations and inferences to ascertain the correspondent changes of wind and weather to be expected after change in the marine barometer.

At the Cape of Good Hope, which is nearly in the same latitude, the mercury rises with the fresh gales that blow there from the SE in the summer season. The weather that accompanies these south-east winds, is nearly similar in both places; the atmosphere being without clouds, but containing a whitish haze, and the air usually so dry as sensibly to affect the skin, particularly of the lips.

3d. Jan. 22. Three degrees east of the Archipelago, the mercury fell with some rapidity down to 29,65 with the wind from ESE. It was eight o'clock at night, and we prepared for a gale from that quarter; but at ten, the wind suddenly shifted to WNW, coming very light off the land. On its veering gradually round to SSW, clear of the land, at noon. 23d, it freshened, and the weather became thick; yet the mercury had then risen to 29,84, and at eight in the evening to 29,95, though the wind then blew strong. It continued to rise to 30,16 as the wind shifted round to SE, and fine weather came on; but on the wind passing round to ENE and NNE, which was off the land, the mercury fell back to 29,73, though the weather was fine and the wind moderate. On a sudden shift of wind to the SW, a fresh breeze with hazy weather, it again began to ascend, and a similar routine of wind, producing nearly the same effects upon the barometer, again took place. The effect of sea winds in raising the mercury, in opposition to a strong breeze, and of land winds in depressing it, though they were light, was here exemplified in two remarkable instances.

4th. In the neighbourhood of the Isle of St. Francis of Nuyts, longitude 133½° east of Greenwich, we experienced a considerable change in the barometer. For nine days in January and February the wind continued to blow constantly, though moderately, from the eastward, mostly from the SE. It appeared like a regular trade-wind or monsoon, but so far partook of the nature of sea and land breezes, as

commonly

Observations and inferences to ascertain the correspondent changes of wind and weather to be expected after change in the marine barometer.

commonly to shift more to the southward in the day, and to blow more from east and NE in the night. The weather was very hazy during these nine days; so much so, that for six of them no observation of the sun's altitude, worthy of confidence, could be taken from the sea horizon, although the sun was sufficiently clear; and in the whole time, the mercury never once stood so high as 30 inches, but was frequently below 29,70. I considered this to be the more extraordinary, as settled winds from the eastward, and especially from SE, had before made it rise and stand high upon this coast, almost universally, even when there was a considerable degree of haze. The direction of the south coast, beyond the Isle of St. Francis, and even abreast of it, was at that time unknown to me; but I then suspected, from this change in the barometer, that we should find the shore trending to the southward, which proved to be the case. The easterly winds, then, whilst they came off the sea, caused the mercury to rise upon the south coast; but in this instance that they came from off the land, they produced a contrary effect; but it is to be observed, that the most hazy part of the time, and that during which the mercury stood lowest, was two days that the wind kept almost constantly on the north side of west, more directly off the land: its height was then between 29,65 and 29,60.

The haze did not immediately clear away on the wind shifting to the westward; notwithstanding which, and that the new wind rose to a strong breeze, and was accompanied with squalls of rain, the mercury began to ascend, and had reached 29,95 when the squalls of wind and rain were strongest; the direction of the wind being then from SSW. On its becoming moderate, between SSW and SSE, the mercury ascended to 30,14, and remained there as long as the wind was southwardly.

5th. Going up the largest of the two inlets on the south coast, in March, we were favoured with fine fresh breezes from SSW to SSE, sometimes with fine, sometimes with dull weather, the mercury rising gradually from 30,08 to 30,22. In twenty-four hours afterwards, it fell below 30 inches, and a light breeze came from the northward, off the land, with finer weather than before. The mercury continued to fall to 29,56, where it stopped; the wind having then ceased to blow steadily from the northward, and become variable. In twenty-four hours more, the wind set in again to blow fresh from the southward, the mercury having then returned to 29,94, and it was presently up to 30,22 and 30,28. It kept nearly at this height for several days that the southwardly wind blew fresh, but on its becoming lighter, and less steady in its direction, the mercury descended; and in the calm which followed, it had fallen to 29,90. This example affords clear proof of a fresh wind from the sea making the mercury rise, whilst a light wind off the land, with finer weather, caused it to descend.

Observations and inferences to ascertain the correspondent changes of wind and weather to be expected after change in the marine barometer.

6th. The calm was a prelude to a fresh gale; but the mercury began to rise at eight in the evening when it had just sprung up; by the next noon it was at 30,10 when the wind blew strongest, and in the evening at 30,22. This gale began as gales usually, if not always do upon this coast, in the north-west quarter, and shifted round to SW and SSW; but quicker than I have generally seen them: there was no rain with it, nor was the atmosphere either very hazy or cloudy*. The mercury continued to rise till it had reached 30,25, and then was stationary as long as the wind remained between south and west; but on its veering round to the eastward of south, a second rise took place, and for forty hours the mercury stood as high as 30,45, the wind being then between SE by S and east: the weather was very dull and hazy during the first half of these forty hours, but finer afterwards. As the winds between SE by S and east slanted off the main land, this example seems to be in opposition to the 4th, and leads me to think, that it might have been the very extraordinary kind of haze, and perhaps some peculiarity in the interior part of the land abreast of the Isle of St. Francis that in part occasioned the fall of the mercury with south-east winds; as much, perhaps, as the circumstance of the wind coming from off the shore.

After this rise in the mercury to 30,45, it fell gradually; but, for thirteen days, kept above 30 inches, the winds being generally between SE and SW, but light and variable, and the weather mostly fine.

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* I afterwards learned from Captain Baudin, that this gale was much heavier in Bass' Strait than we felt it at Kanguroo Island.

Observations and inferences to ascertain the correspondent changes of wind and weather to be expected after change in the marine barometer.

7th. North-eastwardly winds, off the land, were the next that prevailed; they were light, and accompanied with cloudy weather and spitting rain. The mercury fell to 29,70, and remained there till the wind shifted to the west and southward, when it began to rise, and in two days was up to 30,42. At that time we were off the projection marked H. in the chart, in 139½° east longitude; the wind had then veered to the south-eastward along the shore, with a steady breeze, and the mercury remained nearly stationary so long as it lasted; but on the wind dying off, and flawing from one side to the other, it descended quickly to 30 inches. A breeze then sprung up at NW, which, within twenty-four hours, shifted suddenly to SW, and blew a gale which had near proved fatal to us. It was accompanied with rain and very thick weather, and lasted two days; by which time, the mercury had descended to 29,58.

8th. In Bass' Strait, for several days in the month of April, the mercury stood above 30,40 with the wind from the south and eastward, sometimes blowing fresh: the weather generally fine. It then fell half an inch in eight hours, and a wind set in soon after from the north-westward which continued four days, blowing moderately, with cloudy weather, and sometimes a shower of rain; the mercury remaining stationary between 29,83 and 29,89. On this second wind dying away, a strong breeze sprung up which fixed at WSW with squally weather; but for three days no alteration took place in the barometer, until the wind shifted to NW and north, and the mercury then descended to 29,52, though the weather was finer, and wind more moderate than before.

9th. Passing along the south coast of Australia the second time, we experienced light winds from the sea for forty hours in D'Entrecasteaux's Archipelago, in the month of May: they were variable between WSW and SSE with dull cloudy weather, and the mercury stood very high, being up to 30,50 most of the time. The wind then came round to N by E and NNW; previously to which, the mercury began to descend, and it kept falling for two days till it reached 30,19, though the weather was not so cloudy as before, and the wind was equally light. On the wind veering to west and WSW the mercury rose to 30,25; but it now came on to blow fresh, with squally thick weather, yet the mercury continued nearly stationary for twenty-four hours, appearing to be kept up in consequence of the wind having shifted round to SSW, more directly from off the sea. On its increasing to a gale, however, there was a pretty rapid descent in the barometer to 29,96; but the ascent again was equally rapid, and to a greater height, on the wind becoming moderate. In a short calm that succeeded, the mercury stood at 30,42, but on the wind setting in from the north, which was from off the land, it fell to 30,25, and remained there two days: we had then reached Bass' Strait.

Observations and inferences to ascertain the correspondent changes of wind and weather to be expected after change in the marine barometer.

From these examples upon the south coast, it appears, generally, that a change of wind from the northern, to any point in the southern half of the compass, caused the mercury to rise, and a contrary change to fall; and that the mercury stood considerably higher when the wind was from the south side of east and west, than, in similar weather, it did when the wind came from the north side; but, until it is known what are the winds that occasioned the mercury to ascend, and what to descend, upon the other coasts of Australia, it will probably be not agreed, whether it rose in consequence of the south winds bringing in a more dense air from the polar regions, and fell on its being displaced by that which came from the Tropic;—or whether the rise and higher standard of the mercury was wholly, or in part, occasioned by the first being sea winds, and the descent because those from the northward came from off the land.

The height, at which the mercury generally stood upon the south coast, seems to deserve some attention. It was very seldom down to 29,40, and only once to 29,42. Of one hundred and sixty days, from the beginning of December to May, it was nearly one-third of the time above 30 inches; and the second time of passing along the coast, from the 15th of May to the 1st of June, it only descended to 29,96, and that for a few hours only, its average standard for these sixteen days being 30,25. Upon the eastern half of the coast, beyond Cape Catastrophé, in March, April, and May, the mercury stood higher than it did on the western half in December, January, and February: the average standard of the first was 30,09, but that of the latter

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latter only 29,94. At the Cape of Good Hope, the mean height in the barometer, during eighteen days in October and November, was 30,07.

The marine barometer on board the Investigator, supplied to the astronomer by the Board of Longitude, was made by Nairne and Blunt, and had, I believe, been employed in one or more of the voyages of Captain Cook, and perhaps in that of Captain Vancouver. I suspect, that it was not suspended so exactly in the proper place, as the latter instruments of these makers probably are; on which account, the motion of the ship caused the mercury to stand too high; and perhaps one or two-tenths of an inch might be deducted with advantage from the heights taken at sea, but I think not when the ship was lying steadily at anchor in the harbour. The barometer stood in my cabin, and the height of the mercury was taken at day-break, at noon, and at eight in the evening, by the officer of the watch; as was also that of the thermometer.

The general effects produced upon the barometer by the sea and land winds, on the east coast of Australia, will be learned from the following abridgment of our meteorological journal:

1*st*. In the run from Cape Howe, in 37½° south latitude, to Port Jackson, in 34°, once in the month of May, and once in June, I found that the mercury descended with light winds from north, NW, west, and WSW; whilst during fresh breezes from south and SW it ascended, and stood considerably above 30 inches; with the wind at NE and NNE it also kept above 30 inches, but not so high, nor did it rise so fast, as when the wind was from SSW. From between south and east, the winds did not blow during these times. This example does not differ so much from those on the south coast as to be decisive of any thing.

2*d*. The observations made during a stay of ten weeks at Port Jackson, in May, June, and July, 1802, are more in point than almost any other. Strong eastwardly winds were very prevalent at that time, and were almost always accompanied with rain and squalls; yet this weather was foretold and accompanied by a rise in the barometer, and the general height of the mercury during their continuance was 30,20: higher if the wind was on the south side of ESE, and lower if on the north side of east. The winds from south and SSW, which blow along the shore, kept the mercury up to about 30,10, when they were attended with fine weather, as they generally were; but if the weather was squally, with rain, it stood about 29,95. During settled winds from between WNW and SW, with fine weather, the mercury generally stood very low, down at 29,60[*]; and what is more extraordinary, when these winds were less settled, and the weather dull, with rain occasionally falling, the range of the mercury was usually between 29,80 and 30,10; nearly the same as when the wind was at SSW with similar weather; the reason of which may probably be, that at some distance to the southward these westwardly winds blew more from the south, and were turned out of their course, either by the mountains, or by meeting with a north-west wind farther to the northward.

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The winds from north and NW blew very seldom at this time: the mercury fell on their approach.

To the state of the mercury during our second stay at Port Jackson, in July, 1803, and part of June and August, it is not in my power to refer, as I have not been able to obtain that part of my journal from General De Caën.

The effects of some winds upon the barometer in this 2d example, are considerably different to what they were upon the south coast. The wind at WSW or SW with fine weather, had always caused the mercury to rise and stand high, and those from the NE to fall; whereas here, the effects of those winds were almost directly the reverse. The winds from SSW, SE, and as far as east, made it rise on both coasts, with the exception of the 4th example on the south; and from between north and WNW the mercury fell in both cases and stood low.

3d. Steering along the east coast, from Port Jackson to the northward, in July, we had the winds usually be-

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[†] My friend Colonel Paterson, F.R.S. commander of the troops at Port Jackson, in judging of the approaching weather by the rise and fall in his barometer in the winter season, told me, that he had adopted a rule directly the reverse of the common scale. When the mercury rose high, he was seldom disappointed in his expectation of rainy, bad weather; and when it fell unusually low, he expected a continuance of fine, clear weather, with westwardly winds.

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tween south and SW, and sometimes WSW, the mercury being nearly stationary at 30 inches; but meeting with a spurt of the south-east trade wind in latitude 24°, we found it rise to 30,30 for two days. A westwardly wind brought it back to 30 inches for a short time; but on the trade wind finally setting in, it fixed itself between 30,20 and 30,30, as long as the wind preserved its true direction.

4th. The month of September, 1802, and the greater part of August and October, we spent upon the east coast between the latitudes 23° and 17°. The south-east trade is the regular wind here, but we had many variations. Whilst the trade prevailed, the average standard of the mercury was 30,15, and the more southwardly it was, and the fresher it blew, the higher the quicksilver rose, though it never exceeded 30,30. When the trade wind was light, it was usual for a breeze to come off the land very early in the morning, and continue till eight or nine o'clock; but these temporary land winds did not produce any alteration in the mercury, which kept at these times about 30,10. When the trade wind veered round to ENE, or more northward, which was not seldom, the mercury ranged between 30 inches and 30,10; and when a breeze from north or N by W prevailed, which was the case for a considerable part of twenty days we remained in Broad Sound, its height was something, but not much less. These northwardly winds I take to have been the north-east wind in the offing; which had been partly turned, and in part drawn out of its direction, by the peculiar formation of this part of the east coast. There are but few instances of any steady westwardly wind prevailing; when such happened, they were generally from the north side of west; and at these times the range of mercury was between 29,95 and 30,05, which was the lowest I at any time saw it on this portion of the east coast.

The barometer was of great service to me in the investigation of this dangerous part of the east coast, where the ship was commonly surrounded with rocks, shoals, islands, or coral reefs. Near the main land, if the sea breeze was dying off at night, and the mercury descending, I made no scruple of anchoring near the shore; knowing that it would either be a calm, or a wind would come off the land; but

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if the mercury kept up, I stretched off, in the expectation that it would freshen up again in a few hours. Amongst the barrier reefs, when the wind was dying away, the barometer told me, almost certainly, from what quarter it would next spring up. If the mercury stood at 30,15, or near it and was rising, I expected the proper trade wind; and if higher, that it would be well from the southward, or would blow fresh; and if it was up to 30,30, both. The falling of the mecury to 30,10 was an indication of a breeze from the north-eastward; and its descent below 30 inches that it would spring up, or shift round to the westward. This regularity of connection between the barometer and the direction of the wind, is perhaps too great to be expected at a different time of the year; and it is probable, that we should not have found it continue so strictly, had our stay amongst the barrier reefs been much prolonged.

5th. Leaving the east coast in the lat. 17° south, we steered off to the northward for Torres' Strait, in the latter part of October. As we advanced northward, I found the mercury stand gradually lower with the same kind of wind and weather. The difference was not material till we reached the latitude 13°, but afterwards, the south east wind which had before kept the mercury up to 30,15, then permitted it to fall to 29,90; and the winds from ENE and NNE to 29,85. Was this alteration owing to the want of density in the air brought in by the south-east winds, in this lower latitude?—to our increased distance from the land?—or was it, that the south-east wind was no longer obstructed by the coast, having a passage there through Torres' Strait?

The difference between the height of the mercury, during a north-east and a south-east wind, was much less here than before, although the weather was most unfavourable during the time of the north-east wind, and should have increased the difference in their effects. Was this owing to the general approximation to that equality which has been observed to take place in the barometer, in very low latitudes?—or that the north-east wind, still meeting with resistance from the coast, had one cause for keeping up its power, which the south-east wind had lost?

In a general summary of the winds on the east coast, those

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those that came from between south and east caused the mercury to rise and stand highest, as they had also done upon the south-coast, with the exception of the 4th example. The winds from NE kept the mercury up above 30 inches on the east coast, and caused it to rise after all other winds except those from the south-eastward; but on the south coast, the mercury fell with them, and stood considerably below 30 inches; because, as it appears to me, they then came from off the land. During north-west winds, the mercury stood lower than at any other time upon both coasts; and on both they came from off the land.

Moderate winds from the south-westward, with fine weather, caused a descent of the mercury on the east coast; and during their continuance it was much lower than with winds from the north-eastward; but upon the south coast it rose with south-west winds, and stood much higher than when they came from the opposite quarter. For this change I cannot see any other reason, than that the wind, which blew from the sea upon one coast, came from off the land in the other.

Although the height of the mercury upon the south coast of Australia was, upon the average, considerably above the medium standard 29,50, it was still greater upon the east coast: I cannot fix it at less than 30,08 or 30,10, whereas upon the south coast, I should take it at 30 inches; both subject to the probable error of one or two-tenths of an inch in excess. This superiority seems attributal to the greater prevalence of sea winds upon the east coast, and particularly of those from SE, which, *when all other circumstances are equal*, I have observed to raise the mercury higher than any other on this side of the equator, analogous to the effect of north-east winds in the northern hemisphere; and perhaps also, the superiority may be in part owing to the east coast having a more regular chain of higher mountains running at the back of, and parallel to it, which presents a greater obstruction to the passage of the wind over the land, than it meets on the south coast.

(To be Continued.)

VI.

Letter from a Correspondent, on the Exhibition of the Invisible Girl.

To Mr. Nicholson.

Sir,

Bristol, Jan. 9, 1807.

Confirmation of the former explanation of the Invisible Girl.

THE account of your correspondent X. of the manner in which the Invisible Girl amused the lounging public, exactly agrees with one which I sent to Mr. Walker, of Conduit Street, about two years ago, except that X. seems to have failed, as I did, in discovering the mode by which she saw the company; for one cannot be satisfied with being told, that "a small hole closed with glass is left through "the tunnel and side-wall of the room;" having carefully examined the room of that exhibited at Bristol, and ascertained that there could be no such aperture. Besides, we know that to see through any hole of a very small size the division must be nearly as thin as a sheet of paper, and a hole through a tunnel and side-wall must have been very long, indeed much too long to see people through. As my friend never answered that letter, I concluded that he either doubted of my account being the true one, or that he could not explain satisfactorily how the view of the company was acquired at Charles's exhibition; although he would not have been long at a loss to invent some expedient, had it been worth his while.

Question, How did she see the company?

Account of an exhibition at Bristol.

In fact, thinking it might hurt the harmless exhibitors, or lessen the amusement of the public, I desired that account might not be published, unless necessary to prevent superstitious uses being made of the trick; and, after all, we lose by all these discoveries when made public, much innocent pleasure, as I well remember was the case when Mr. Thickness unveiled some such exhibitions. That which I saw at Bristol and Bath had a loose rail with eight legs; seven of which the operator always removed from their places to blunt suspicion, but the eighth I always found immovably fixed, and that was ever the leg toward the closet where the lady sat who directed us.

His rail also was covered opposite the mouths of his trumpets with stained paper; but you could feel the vibration on the holes when any one answered, and peoples' hands had a little indented them by accidental pressure. As to a small camera, I do not think it was ever used here, or at all necessary for the lady, as a yard of tube with a trumpet mouth would have answered all the purposes: as, however, you have been at the pains of satisfying the general curiosity in so handsome a manner, excuse me if I request your correspondent to complete the instrument by disclosing what he actually knows of the mode of complete vision by direct or indirect reflection; being always,

Sir,
Your's,
G. C.

P.S. You have omitted *three letters* in the diagram of the perspective lines.

VII.

Description of a new permanent Compensation-Balance for a Time-keeper. By Mr. WM. HARDY*.

Description of two compensation balances at present in use.

WE have at present two compensation-balances; one sort consists of several slips of brass and steel soldered, or fluxed together, and disposed in form of two S S's on the balance, but this is almost now out of use. The other is a steel balance, having a rim of brass fluxed upon its outside, and cut open in two or three places, with sliding weights on the rim, to increase or diminish the effect of the balance. The nature of the balance (the only one now in use) is well known, as well as its defects, which it is unnecessary for me to state at this time, as I shall have a better opportunity of pointing them out at large, should I be ordered to attend the Society.

Objections.

Instead of this uncertain way of constructing a balance, which never continues long in the same state, but requires to be adjusted every time the watch wants cleaning; I have rejected this mode altogether, and have contrived a method of applying the direct expansion of metals, which I find by experience to be constant and permanent in its effects.

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* Communicated to the Society of Arts, who voted a reward of thirty guineas.

The new balance described.

My balance consists of a flat steel bar, which forms its diameter. Beneath this steel bar are two metallic rods, secured at one end by a stud, formed out of the steel bar, and the other end acting on the short end of a lever, formed out of the other end of the same steel bar, being made to spring at the place where the centre of the lever would fall; to this lever is fastened a small cylindrical stem of brass, upon which a small globe of brass slides or screws; there is also a screw passing through the stem, to serve to regulate to mean time. Another metallic bar, equal and similar, and furnished like the other, but reversed in position, is placed parallel to it.

Mode of acting.

Action of the new balance explained.

When the whole balance is heated, the metallic rods will push forward the short ends of the levers, and which quantity will be just equal to the difference of the expansion of the two metals. Suppose the short ends of the two levers to be each equal to 1, and the long ends of the levers to be each equal to 20, then it is evident that the motion of each globe will be twenty times the excess of the steel bar and metallic rods nearer to the centre of the axis of the balance, than before the expansion took place; and, what is a very grand and necessary property in the motion of the two globes, they will always move directly to the axis of the balance; that is, their action will be constantly in a plane, passing through the axis of the globes and axis of the balance. To increase or diminish the expansion of the balance, will be only to slide or screw up or down the globes upon their stems, until the balance produces the desired effect.

Additional Remarks on the Balance now in Use.

Manner in which the common expansion balance is supposed to fail.

The rim of brass and steel of the common balance, however intimately connected when first fixed together, are by every change of temperature endeavouring to break the connection, and do by little and little tear themselves asunder, at least in a partial degree, for the fracture is often visible, so that the balance has nothing permanent in its nature. New adjustment is necessary much oftener than the instrument requires cleaning: but that adjustment is of no duration; for, as the pores are more torn than at first, the balance becomes worse and worse, and at last quite useless for what it is intended.

The new balance has no soldered or welded surface.

I make use of the direct expansion of metals; for the bars of my balance are independent of each other. They are connected only at the extremities, and the excess or difference of the expansion of the two metals is communicated to the short ends of the two spring levers. Its durability can therefore no more be doubted than that of the gridiron pendulum, where the direct expansion of metals produces the desired effect.

Its weights move nearly in a diameter.

The two globular weights were described in my last letter as moving constantly in the same plane, which passes through their centres and the axis of the balance; and I should have added that, as to sense, they also move in the same right-line which passes through the centres of the globes, and cuts the axis of the balance at right angles; for the versed sine of a very small arch, or the difference between the radius and co-sine, is in this case a quantity so small that it cannot be perceived; and however we increase or diminish the expansion of the balance, or whatever may be the degree of temperature, it still retains this admirable property, namely, that the two spherical weights move not only in the same plane in a strict mathematical sense, but also in the same right line in a physical one. This quality, united with the direct motion of the brass bars, renders the motion of the globes simple and uniform, and therefore the effect (depending on such simple and direct causes) is regular and certain.

A noxious vibration of the weights in the common balance.

The common balance, when in motion, causes the weights to fly off or recede from the axis of the balance, and this flying off will increase and diminish with the arch of vibration in the balance: for, as there is nothing to brace the rim at the extremity of which the weight is suspended; as the arch of vibration increases, the weight and rim are thrown outward as much as the centrifugal force of the weight exceeds that of the elasticity of the rim. And as the arc of vibration diminishes, and consequently the centrifugal force, the weight is thrown inward by the elasticity of the rim.

My weights or spheres are firmly braced in every degree of temperature, and consequently not influenced in the smallest degree by any change in their centrifugal forces; therefore, in every respect, this balance may be considered as permanent.

The new balance preserves its equilibrium.

The great difficulty in constructing a balance, and in applying the direct expansion of metals, is to contrive it so as that it shall preserve its equilibrium in every degree of temperature, and also admit of having all its parts made perfectly equal and similar by mechanical means. Both these important problems I have solved, by the introduction and application of a different principle from any yet used in the construction of the balance of a timekeeper; and I am fully satisfied, from a variety of experiments which I have made, that by this total change of system, I have made a higher step towards the perfection of time-keepers, than has been effected by any other means that have come within my knowledge.

*Letter to the Secretary, by the Editor**.

Dear Sir,

I take the liberty to express my opinion of the compensation-balance, which Mr. Hardy has submitted to the consideration of the Society of Arts. I think it a very excellent contrivance: the following are some of the reasons which, I presume, will entitle it to the approbation of that respectable Institution.

Advantages of Mr. Hardy' balance.

1st.—The invention of confining the flexure of the steel bar to a small part near the end is new, and no less remarkable for its ingenuity and simplicity, than for the steady effect it produces.

2d.—The whole combination is particularly firm; and as the workmanship depends upon faces which are either plain or turned in the lathe, it can very easily be manufactured without requiring uncommon skill in the workman.

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* The useful and patriotic society to which this letter was addressed through their Secretary, is always ready to receive communications respecting the subjects proposed for their consideration.—N.

3d.—As it has neither working surfaces of contact, nor joints nor levers, it will regularly obey the minute changes of temperature, and will not act by jerks or starts.

Explanation of the mode of action in bars of brass and steel fused or soldered together.

4th.—In the expansion-bar consisting of two metals, connected longitudinally by soldering or otherwise, the differences of length between them, when heated or cooled, are found to produce a bending of the whole bar, which is more the thinner its component parts. At the very surface of contact, and at a considerable distance on each side of that surface in thick bars, the principal effect must consist in what workmen would call wire-drawing the one metal, and upsetting the other. It is reasonable to think that this process must affect the properties of a balance so constructed, and cause it to deviate in the course of time from its original adjustment. This objection to the common expansion-balance appears to be obviated in Mr. Hardy's invention. The flexure of the brass takes place through its whole length, in a regular manner, and is in quantity but small; and the flexure in the reduced parts of the steel bar will be equally slight, if the thickness of that part be made to bear the same proportion to its length. Hence, and upon the whole, it may be concluded that when once it is adjusted, it will not alter, and that in all changes of temperature it will be similarly affected, and will return to its original figure whenever the first temperature is restored.

Hardy's balance has not the same faults.

It is easily made.

5th.—Artists will probably consider it as a desirable property of the present instrument, that the adjustments for temperature being in lines nearly parallel to the verge, will have no practical effect in deranging the adjustment for position.

I have the honour to be,
Dear Sir,
Your most obedient servant,
WILLIAM NICHOLSON.

Soho-square, March 7th, 1805.
To CHARLES TAYLOR, ESQ.

A certificate, dated March 6th, 1805, was received from Mr. Alexander Cuming, of Pentonville, stating that he had seen Mr. Hardy's expansion balance; that in his opinion it has considerable merit, and promises to act uniformly, steadily, and permanently.

Reference to the Engraving of Mr. William Hardy's Permanent Balance, Plate IV. *Fig.* 4, 5; *expressing in inches and decimal parts of an inch, the dimensions of the several pieces.*

Fig. 5. A A. Two globes which slide on the cylindrical stems of two upright levers, and are fastened by screws, by which the effect of the expansion is increased or diminished.

Figure and diagrams of the new balance.

C C. Two equal and similar screws, by which the watch is adjusted to mean time.

D D. The verge or axis of the balance.

E E. The combination of the steel bar with the brass bars.

Fig. 6. S S. The steel bar, whose length is	1.600
Its breadth	0.232
Its thickness	0.032
B B. Two similar and equal brass bars, in length each	1.470
In breadth each	0.078
In thickness each	0.032
Length of the two springs formed out of the steel bar	0.030

VIII.

Description of an expanding Band Wheel to regulate the Velocity of Machinery. By MR. ANDREW FLINT*.

The relative velocity of machinery connected by band is not variable.

IN the usual method of connecting machinery, by a band running over two wheels or riggers; it is obvious that the relative velocity of the wheels is in the inverse ratio of their diameters; and these diameters always remaining the same, no alteration of the velocity can be obtained, but by a corresponding variation in that of the moving power applied.

Invention of the author to make it so.

To enable the artizan to regulate the velocity of his machinery at pleasure, the moving power remaining as before, or to retain the same motion, with an alteration in that of the applied force, is the purpose of the invention, the models of which are now laid before the Society. In this model are shewn two methods of attaining this desirable object; in both, the periphery of the band-wheel is divided into any convenient number of parts, according to the size of the wheels, (in this case twelve) which may be placed at any given distance from the centre of the wheel, (within the limits of the machinery) and thus, by enlarging the circumference of one band-wheel, while the other is equally diminished, to alter the relative velocity of each at will. These parts of the periphery, which I term V's, and are marked by the letters *i i i i i*, &c. Plate III. are confined to move in grooves, cut in the large wheels A and B, *Fig.* I. and II. in the direction of their radii, and are moved by a spiral thread in the small wheel C, which thread takes in the teeth of the racks on which the V's are fixed. A part of the shaft on which the wheel A is fixed, is made circular, to admit the small wheel C to turn round independently of the other, and thus to extend or contract the racks and V's in *Fig.* III.—*Fig.* IV. is a section of part of the rigger, in which the letters refer to the same parts as in *Fig.* I. and II.

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* Society of Arts, Vol. XXIII. A premium of fifty guineas was awarded for this invention.

In the wheel D, the same effect is produced by the screws, *e*, *e*, &c. which are made alternately right and left handed, and turn with equal motions, by means of the equal bevil-wheels *f*, *f*, &c. fixed at their ends near the axis of the wheel. *Fig*. V. is a section of the same.

The wheel C, *Fig*. I. and II. is moved round the shaft *d* by the pinion *g*, on the axis of which is fitted occasionally a winch. The screws of the wheel D, *Fig*. III. may be also turned, by means of a winch applied to their projecting heads *h*, *h*, *h*. It is proper to notice that the number of the screws must always be equal.

ANDREW FLINT.

Goswell Street, *London*.

IX.

Account of a Discovery of native Minium. In a Letter from JAMES SMITHSON, ESQ. F.R.S. *to the Right Hon. Sir* JOSEPH BANKS, *K.B. P.R.S.*[*]

MY DEAR SIR,

Character and habitudes of native minium

I BEG leave to acquaint you with a discovery which I have lately made, as it adds a new, and perhaps it may be thought an interesting, species to the ores of lead. I have found *minium* native in the earth.

It is disseminated in small quantity, in the substance of a compact carbonate of zinc.

Its appearance in general is that of a matter in a pulverulent state, but in places it shows to a lens a flaky and crystalline texture.

Its colour is like that of factitious minium, a vivid red with a cast of yellow.

Gently heated at the blowpipe it assumes a darker colour, but on cooling it returns to its original red. At a stronger heat it melts to litharge. On the charcoal it reduces to lead.

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[*] Philosophical Transactions for 1806.

In dilute white acid of nitre, it becomes of a coffee colour. On the addition of a little sugar, this brown calx dissolves, and produces a colourless solution.

By putting it into marine acide with a little leaf gold, the gold is soon entirely dissolved.

When it is inclosed in a small bottle with marine acid, and a little bit of paper tinged by turnsol is fixed to the cork, the paper in a short time entirely loses its blue colour, and becomes white. A strip of common blue paper, whose colouring matter is indigo, placed in the same situation undergoes the same change.

The very small quantity which I possess of this ore, and the manner in which it is scattered amongst another substance, and blended with it, have not allowed of more qualities being determined, but I apprehend these to be sufficient to establish its nature.

It seems to be produced by decay of galena.

This native minium seems to be produced by the decay of a galena, which I suspect to be itself a secondary production from the metallization of white carbonate of lead by hepatic gas. This is particularly evident in a specimen of this ore which I mean to send to Mr. Greville, as soon as I can find an opportunity. In one part of it there is a cluster of large crystals. Having broken one of these, it proved to be converted into minium to a considerable thickness, while its centre is still galena.

I am, &c.

JAMES SMITHSON.

Cassell in Hesse,
March 2d, 1806.

X.

An Account of a new semi-metallic Substance, called Menacane, and its Ores. By the late G. MITCHEL, *M. B.*[*]

The natural history of menacane has been little attended to.

SINCE the discovery of Menacane by Mr. Gregor, the distinguishing properties of the peculiar metallic substance it contains have been so fully developed, and satisfactorily ascertained, by the united exertions of Kirwan, Klaproth, Vauquelin, and Lampadius, that little is left to wish for, so far as the chemical characters are concerned. As an object of natural history, it has, as yet, been little attended to. It is therefore hoped, the following attempt, to supply in some measure that deficiency, so far as the present data allow it, will prove acceptable to the naturalist. It is scarcely necessary to observe, that I follow Werner's method most exactly: as it is to him that we are indebted for the successful vindication of Mineralogy, as an independent province in the fœderal state of natural history; and which acknowledges, in Chemistry, the powerful and indispensable ally, not the imperious and arbitrary law-giver.

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[*] Irish Transactions, Vol. X.

Five species.

Of the genus Menac we are already acquainted with five species or ores. It is, however sufficiently probable, that several new species will, at no distant period, be added to the list; and that this metal is more widely distributed, and more generally diffused, and plays, perhaps, a more important part, than is at present suspected.

MENAC, GENUS.

Tribe of Rutile .... 1. Rutile.
2. Rutilite.
Tribe of Menacane.. 3. Nigrine.
4. Menacane.
5. Iserine.

FIRST SPECIES.

RUTILE*.

Species I. Rutile.

Titanite of Kirwan.
Rutil of Werner.
Sagenite of Saussure.

EXTERNAL CHARACTERS.

External characters.

The colour varies from light hyacinth to dark brownish red. Is found crystallized. 1. In right angled four-sided prisms, acuminated by four planes, which are set on the lateral planes. 2. In six-sided prisms, which are said sometimes to exhibit a tendency to a six-sided acumination. 3. In acicular and capilliform crystals, whose regular shape is no longer determinable, and which are, moreover, strongly compressed.

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* Probably the anatase of Hauy is a variety of Rutile.—R. J.

The crystals are longitudinally sulcated, often very deeply; are commonly small, and very small, rarely middle sized. The acicular are often fascicularly aggregated: the capilliform crystals are often in a singular manner reticulated, the interstices forming equilateral triangles; exteriorly, shining and moderately glistening; interiorly, glistening; the lustre adamantine.

The principal fracture is foliated with a two-fold cleavage, cutting each other at right angles: the transverse fracture is imperfect and minute conchoidal. The fragments are cubical. It sometimes exhibits slender, columnar, distinct concretions. Is usually translucent, sometimes only translucent at the edges. Hard. Brittle. Gives a pale orange yellow streak. Is easily frangible. Heavy, in an inferior degree, about 4,200.

OBSERVATIONS.

Observations on Rutile.

The larger crystals, particularly those from Hungary, are often curved, have frequent transverse rifts, are sometimes broken entirely across, the ends removed to some distance from one another, and the interstices filled up with the substance of which the matrix consists: sometimes two crystals meet under an angle more or less obtuse, and are joined like the corner of a frame. The crystals are, moreover subject to great irregularities, are seldom fully crystallized, and, therefore, rarely acuminated; the four-sided prisms are often slightly rhomboidal; the six-sided prisms, from Hungary, are usually dilated, and seem composed of accumulated acicular crystals, from whence arise the columnar distinct concretions; the six-sided prisms, from France, are said to originate from the truncation of two opposite lateral edges of the four-sided prism; the capilliform crystals are sometimes coloured green, from chlorite earth. By some authors, this fossil has been said to resemble red silver ore; but the slightest acquaintance with the oryctognostical characters is sufficient to shew the difference; a geognostical character also furnishes us here with an easy means of distinguishing this fossil from other ores of a red colour. Rutile is generally of cotemporaneous formation with its associated fossils; whereas red silver ore, red orpiment, &c. being formed in veins, are always of later formation than the rock on which they are seated. Some systematic writers have confounded it with rubellite, with which it has scarcely two characters in common.

CHEMICAL CHARACTERS.

Chemical characters of rutile.

Without addition, or even with phosphoric salts, it is infusible by the heat of the common blow-pipe; with borax or alkali, it affords a hyacinth red transparent glass; with the heat excited by pure air, it gives a milk white bead, and suffers a considerable loss of weight. It is insoluble in the mineral acids, before it has been melted with alkali, but yields readily to acid of sugar; is precipitable by acid of galls with a bright red, and by Prussian alkali with an handsome dark green colour. The method of analysis I shall omit, as belonging properly to mineralogical chemistry; the result has shewn that this fossil consists wholly of the calx of Menac.

GEOGRAPHIC DISTRIBUTION.

Geographic distribution of rutile.

This fossil has hitherto been discovered in but few places, and in moderate quantity, principally near Rosenau, in Upper Hungary; in Mount St. Gothard, in Switzerland; in Fischthal, in the high mountains of Saltzburg; near St. Yrieux, in France; in the province of Burgos, in Spain; in the forest of Speysart, near Aschaffenberg, in Franconia; at Beresooskoi, in Siberia; and Olapian, in Transylvania.

GEOGNOSTIC OCCURRENCE.

Geognostic occurrence of rutile.

The Hungarian rutile is found imbedded in a kind of quartz, passing into rock crystal, and forming nests in mica slate; it is therefore of cotemporaneous formation with the rock in which it lies. That from St. Gothard, in Switzerland, occurs partly in those drusy cavities, which are not unfrequent in granitic mountains of high antiquity, lying in or upon the rock crystal, adularia, and foliated chlorite, with which those cavities are lined, and partly dispersed through, or seated in, the scarcely perceptible clefts of one of those nameless chloritic rocks, which abound so much throughout the Alps in general. That from Aschaffenberg is said to occur in granite; that from Saltzburg is found imbedded in massive common tremolite. The rutile from Spain and Siberia is embedded in rock crystal. It would therefore appear that this fossil lays claim to great antiquity, the time of its production falling within the period of the earlier primitive rocks, and that the metal it contains probably surpasses, in that respect, tin, molybdæna, and tungsten, vieing even with iron and manganese *.

The above description has been chiefly taken from an attentive examination of the specimens of rutile existing in the best collections of Vienna and Saxony.

SECOND SPECIES.

RUTILITE.

Species II. Rutilite.

Calcareo-siliceous titan ore of Kirwan.
Titanit of Klaproth.

EXTERNAL CHARACTERS.

External characters.

The colour varies from brownish red to dark reddish brown. Has been hitherto found only crystallized in very rhomboidal four-sided prisms, acutely bevelled at the extremities, the bevelling planes set on the obtuse lateral edges. The crystals are small, and very small, seldom middle sized. Exteriorly, they are shining. Interiorly, glistening, with a resinous lustre. The fracture is imperfect and minute conchoidal,

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* Von Buch has discovered rutile in layers of quartz, in clay slate (Thonschiefer), near Nühlbach, in Saltzburg, in the vicinity of metallic layers, consisting of copper glance, copper pyrites, iron pyrites, nickel, and rarely native copper: also on the mountain Brennkogl, in the valley of Fusch; where it occurs in mica slate, either reticularly aggregated in rifts, or in acicular crystals, accompanied by those singular cylindrically aggregated crystals of foliated chlorite, in venules of almost coeval formation with the rock itself.—Buch's Geognostiche Beobachtungens—R. J.

Rutile has also been discovered by Von Humboldt, on the summit of a mountain near Caraccia, in New Granada, at the height of 1516 toises.—R. J.

choidal, passing into the uneven. The fragments are indeterminately angular, tolerably sharp edged. The transparency varies, from translucent, through translucent at the edges, to opaque. Is semi-hard, bordering upon hard. Brittle. Gives a greyish white streak. Is easily frangible. Not particularly heavy, approaching the heavy (3,500).

OBSERVATIONS.

Observations on rutile.

The lateral planes meet alternately under angles of 135° and 45. From the foregoing fossil it is sufficiently distinguished by crystallization, fracture, inferior hardness, and specific gravity. From grenatite it may readily be discriminated, by the difference in crystallization, fracture, and sort of lustre.

CHEMICAL CHARACTERS.

Chemical characters.

Before the blow-pipe it suffers no change, nor in the heat of a porcelain furnace, when exposed in an earthen crucible; but in a crucible of charcoal it melts to an imperfect black glass, owing to the partial reduction of the metallic contents. With considerable difficulty, and only by repeated digestion, marine acid dissolves a third part of the weight of this fossil, consisting partly of the menac contents. Klaproth, from whom these characters are taken, found it to consist of nearly equal parts menac-calx, silex, and lime, to which Vauquelin joins a large portion of iron calx.

GEOGNOSTIC OCCURRENCE.

Geognostic occurrence of rutilite.

In the mountains of Passau, this fossil is found imbedded in a coarse granular aggregate of felspar and hornblende, and felspar and actynolite; therefore belonging to the genus green-stone, and order of primitive trap. In Norway it occurs in rocks belonging to the same formation, in which the celebrated layers of magnetic iron ore lie, and is associated with hornblende, and several individuals of a tribe not as yet sufficiently examined and described, but which evidently constitute middle links between actynolite and hornblende, and to which the names arendalite and acanticone have been applied. Near Dresden and Brünn it is found dispersed through sienite; and at Galway, in Ireland, in an uncommonly beautiful porphyritic sienite. Hence it appears, that this fossil has only occurred in rocks belonging to primitive trap, or in sienite, the last crystallization which took place within the primitive period, and must therefore be considered as a later production than rutile. Here a consideration of the laws of crystallization countenances the observations on the order in which the primitive rocks follow one another. The rutile, consisting of few and simple elements of cotemporary origin, with a granite, in which rock crystal occupies the place of quartz, and adularia that of common felspar, sufficiently bespeaks a period, when the solution being purer and more tranquil, furnished an earlier and purer crop of crystals; while the confused and irregular crystallization of primitive trap and sienite, together with the greater impurity of the felspar, and very compounded nature of the hornblende and rutilite, indicate an inferior purity of the solution, and, consequently, later precipitation of the crystallized mass.

Species III. Nigrine.

THIRD SPECIES.

NIGRINE.

Nigrin of Werner.

EXTERNAL CHARACTERS.

External characters.

The colour is dark brownish black, passing into velvet black. Is found in larger and smaller angular grains, and pebbles. Externally, moderately glistening. Internally, principal fracture is glistening; the transverse fracture moderately glistening. Lustre, adamantine. The principal fracture is imperfectly foliated, with a single cleavage; the transverse fracture is flat, and imperfectly conchoidal. The fragments are indeterminately angular, and sharp-edged. Perfectly opaque. Semihard. Brittle. Gives a yellowish brown streak. Heavy, in a moderate degree (4,500).

OBSERVATIONS.

Observations on nigrine.

This fossil is readily distinguished from menacane, by its stronger lustre and superior hardness, the colour of the streak, and by its not being in the least magnetic; which also sufficiently distinguishes it from iserine and iron sand*. Being found in company with fragments of rutile of a dark colour, the latter has by most been confounded under the same denomination; but the red colour of the rutile, joined to its perfectly foliated fracture, with a two-fold cleavage, intersecting each other at right angles, and the thence resulting cubical fragments, distinguish it sufficiently from nigrine.

The present description is taken from a specimen I had the pleasure of receiving from Professor Jacquine the younger, of Vienna.

CHEMICAL CHARACTERS.

Chemical characters of nigrine.

The nigrine is infusible per se by the blow-pipe: but, with the assistance of borax, it melts to a transparent, hyacinth red bead: to acid of sugar, it readily yields its medac contents, which furnishes the characteristic precipitate of this genus. Klaproth and Lampadius have given us the constituent ingredients, 8 or 9 per cent. menac calx, and 2 or 1 calx of iron. It is probable, however, that the proportion of menac calx is over-rated; it appearing evident, from the description accompanying the analysis, that there had been no care taken to select the nigrine from the grains of rutile which accompany it.

GEOGNOSTIC OCCURRENCE.

Geognostic occurrence of nigrine.

The nigrine has been hitherto found only at Ohlapian, in Transylvania, in alluvial hills, consisting of yellow sand, intermixed with fragments and bowlders of granite, gneiss, and mica slate, and from which gold is obtained by washing. This gold is the purest found in Transylvania; a circumstance sufficiently indicating, that it belongs to a different, and, consequently, earlier formation, than the usual Transylvanian native gold, which occurs there in clay porphyry, grey wacce, and grey wacce slate, and belongs to the brass yellow variety, from the considerable alloy of silver which it contains. In these stream works, the nigrine is obtained at the same time with the gold, and comes to us intermixed with grains of rutile, oriental garnet, native iron, granite, and common sand; which renders it extremely probable, that this fossil, also, is a native of the primitive mountains.

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* Genuine iron sand must not be confounded with magnetic iron ore in a *sandy form*, which usually passes under that name.

Species IV. Menacane.

FOURTH SPECIES.

MENACANE.

Menachanite of Kirwan.

Menacan of Werner.

EXTERNAL CHARACTERS.

External characters.

Is of a greyish colour, inclining somewhat to iron black. Only met with in very small, flattish, angular grains, which have a rough glimmering surface. Internally, moderately glistening, with adamantine lustre, passing into the semimetallic. The fracture is perfectly foliated, approaching to the slaty. The fragments are indeterminately angular, and sharp-edged. Perfectly opaque. Is soft. Brittle. Retains its colour in the streak. Easily frangible. Heavy, in a moderate degree (4,427).

OBSERVATIONS.

Observations on menacane.

This fossil has been said, but erroneously, to have much resemblance to iron sand, from which it may be easily distinguished by the fracture, lustre, and inferior specific gravity.

PHYSICAL AND CHEMICAL CHARACTERS.

Physical and chemical characters.

Menacane is attractable by the magnet, but much more weakly than iron sand, or magnetical iron ore; it is infusible by the common blow-pipe, or heat of a porcelain furnace, exposed in a coal crucible, but melts, when in contact with a clay one; it also melts quickly to a black bead, before a blow-pipe animated by pure air. The menac contents may be easily extracted by digestion with acid of sugar. Klaproth and Lampadius, about the same time, have shewn, that it consists of nearly equal parts menac and iron calces.

GEOGNOSTIC OCCURRENCE.

Geognostic occurrence.

This fossil has hitherto been only found, accompanied by fine quartz sand, in the bed of a rivulet, which washes the valley of Menachan, in Cornwall. The neighbouring mountains belong to the primitive order, in which, most probably, the menacane formerly constituted a superficial layer; but, by their decomposition, and consequent degradation, by means of rains and floods, the earthy parts have been carried off, and the heavier metallic fragments collected in the valley.

FIFTH SPECIES

Species V. Iserine.

ISERINE.

Iserine of Werner.

EXTERNAL CHARACTERS.

External characters.

The colour is iron black, inclining a little to brownish black. Is found in small obtuse, angular grains, and in pebbles, with a somewhat rough, strongly glimmering surface. Internally, it is shining, with semi-metallic lustre. Fracture is more or less perfectly conchoidal. Fragments are indefinitely angular, and sharp-edged. Perfectly opaque. Hard. Brittle. Retains its colour in the streak. Is heavy, in a moderate degree (4,500).

OBSERVATIONS.

Observations on Iserine.

Of all fossils, this has the strongest resemblance to iron sand; into which, as Mr. Werner first observed, it actually graduates, but may be distinguished from it by the shade of brown in its colour; by its superior external, and inferior internal lustre; by its less specific gravity; but, chiefly, by being only slightly, and that by a powerful magnet, attractable. From nigrine and menacane, it differs sufficiently in fracture and lustre. This, as well as nigrine, was first considered as a particular species by Werner: both which determinations were afterwards confirmed by the analysis.

CHEMICAL CHARACTERS.

Chemical characters.

As in the foregoing species, the menac calx may here be readily extracted by acid of sugar, the residuum being dissolved in aqua regia: on the addition of tartarised tartarin, a lemon yellow powder falls to the bottom, which is tartarised menac; what remains in the solution is iron. Lampadius, to whom we owe the analysis, found that menac and iron are here in a decreasing proportion; the latter amounting to about 20 per cent. A late experiment has shewn him, that iron sand contains the same principles, but, probably, in an inverted proportion.

GEOGNOSTIC OCCURRENCE.

Geognostic occurrence of Iserine.

Hitherto this fossil has been only found in the high Riesen mountains, which separate Silesia from Bohemia, near the origin of the Iser, dispersed through the granitic sand which forms the bed of that river. To what order of rocks it owes its origin is uncertain; but its near affinity to iron sand, which is exclusively an inmate of the flötz trap formation, and the certainty, that this formation was formerly superstratified, at a great elevation, on the Riesen mountains, (as the remains, which form the Buchberg*, and occupy the Schneegruben, sufficiently testify.) render it highly probable, that this fossil, also, may belong to that formation; and, consequently, dates its origin from a much more recent period than the foregoing species of this genus†.

GENERAL REMARKS.

General remarks on me-

These are the only fossils of this genus, with whose characters we are as yet sufficiently acquainted to say, with certainty, that they form distinct species. Between the three latter and iron sand, the intermediate transitions, as between all adjacent fossils, are, probably, innumerable. Were we to take analysis alone for our guide, it would multiply the species without necessity, and lose sight of the intentions of Nature, who does not confine herself to 5 or 10 per cent. of an ingredient; beside, a Klaproth has confessed, that it is not so much the identity and proportion of the ingredients, as the particular state of their combination, (which to us is perfectly unknown,) that determines the nature of the resulting fossil. In addition to those fully determined species, we have been favoured, by Klaproth, with the analysis of a menacaniferous ore, from Aschaffenberg; by Vauquelin, with that of another, from Bavaria: and, by Abildgaard, with that of a third, from Barboe, in Norway; all which differ from the foregoing species, and from one another, in composition, or in the proportion of ingredients; so that it is impossible to determine, with any probability, to what species they belong, from the want of an adequate external description, and account of their geognostic occurrence.

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* The Buchberg (which I enjoyed the invaluable opportunity of examining with my excellent and ever to be regretted friend) is the highest basalt hill in Germany, being 2921 feet above the level of the sea, and the highest basalt, except that small quantity lodged in the cavity of the Schneegruben, which is some hundred feet higher. The hill itself is elevated about 500 feet above the Iser, that washes its granitic basis, and the Iserine is found at some distance below. We could, indeed, discover no trace of it in the basalt of the present hill.—R. J.

† Mr. Gregor (as stated in Nicholson's Journal) has found, that menac is one of the constituent ingredients of basalt; a fact, which adds much to the plausibility of Dr. Mitchell's very ingenious supposition. —R. J.

nacane and its ores.

The masterly hand of Klaproth has further detected this metal, in the iron sand, which accompanies the hyacynths, &c. in Ceylon, and in some of the iron ores of Norway; and Lampadius has lately discovered it, in the iron sand of Hohenstein, near Stolpen, in Saxony, and in that found with the pyrope of Bohemia. Besides these, I have seen, in the imperial cabinet, at Vienna, and some few private collections, ores, said to come from Stiria or Carinthia, and from Bohemia, in which the menac calx probably abounded; as may be conjectured, from the strong shade of brown in the colour, together with the considerable adamantine lustre, both which are strongly characteristic of this genus.

Uses.

The use of this metal is, as will readily be supposed, from its scarcity, and the newness of its discovery, very confined. The rutile, indeed, was, for a length of time, employed to give a brown colour, in the porcelain manufacture of Sevres, near Paris; but, from the difficulty of communicating an equal tint by it, has been since abandoned. The rock crystal, inclosing capilliform crystals of rutile, has been employed as a setting for rings. The precipitates, especially those from acid of sugar, may be employed as water colours; that, by acid of galls, affording a good tile red, and that, with Prussian alkali, an agreeable dark green. The latter, also, communicates a durable colour to silk, as my friend, Lampadius, assures me; perhaps, with proper management, it might be employed to furnish the so much wished for durable green for the printing of cotton. And, lastly, its close connection with some iron ores, and those exactly of the most superior quality, such as the ores of Norway and Stiria, leads naturally to the suspicion, that it may possess some favourable influence upon the manufacture of iron, and, therefore, well deserves the attention of future inquirers. Such are the principal circumstances, at present known, respecting this genus of fossils; time will, doubtless, here, as usual, find much to amend, to correct, and to supply.

XI.

On the Cultivation of Grapes. *By* GEO. CUMBERLAND, *Esq.*

To Mr. NICHOLSON.

SIR, *Bristol, Jan.* 18, 1807.

Useful instructions for defending grapes, and giving them the advantages of sunshine, and the heat of a wall, &c.

PERCEIVING that practicable improvements in all the arts that benefit existence are sure to meet with a favourable reception in your Journal, I trust you will accept the following account of some practices that have lately been successful in the management of fruit trees, particularly the vine on the exposed wall.

Having last year come into possession of some south walls covered with vines that were said not always to ripen so well as might be expected, I was advised to cover them with glass, as the only sure means of securing a very considerable produce; but as that mode was too expensive to suit my circumstances, I resolved to make trial of less costly expedients, and at first turned my thoughts to those bell-glasses blown with a hole in the back, into which the young bunches are introduced very early so as to expand within the glass, and when ripe are severed at the stalk, and delivered at the wider aperture.

The

Useful instructions for defending grapes, and giving them the advantages of sunshine, and the heat of a wall, &c.

The objection to these, however, soon appeared to be, that, even at the glass-houses costing 2s. 6d. each, and upwards, it required at least five years to recover their cost, according to the value of the fruit; next, that in consequence of the hole made in the back they are uncommonly brittle; then, that they can only be applied at a very early season; and lastly, that their colour being obscure, they were on that account less likely to advance the maturity of the bunches (one only of which can be introduced to each) than materials more diaphanous. Finding therefore that to blow them of white glass would nearly double their price, I caused some white flasks of the best flint glass, of about one foot long, to be divided in two by means of the process with a hot coal, and thus I procured out of each flask two covers of the shape of half melons, each of which were capable of covering two bunches at least; of small ones three at a time; and buying them by weight, I found they stood me in only about one shilling each.

These segments I bound with packthread, by making a sling and a tie, so that they were easily attached to the wall by means of a nail, and kept from swinging by a cross thread or two, and thus I covered a great many bunches at all periods, commencing with them when about four or five inches long, and stopping the east side of the glasses with the fresh leaves as I picked them off; for, contrary to the usual practice, I exposed my bunches while green to the sun partially, and some entirely, at a very early period; at a very early period also I began to strip all the leaves from the wall, and to take away all extraneous growth; in fact, I suffered no leaf to remain that touched the wall from the time the vines first came into leaf until the period when the grapes were almost ripe, nor any bunch of grapes at any period to be totally excluded from the sun, laying them particularly open to his declining beams, and only securing them with what care I could from the too piercing rays of noon.

In this manner, under these clear glasses, I exposed several bunches of a sweetwater, growing on a buttress in an angle of about 80 deg. due south, to every ray of sunshine, except the direct ones, and not only did so, but I cleared every leaf away from the wall that approached within a foot each way

of

Useful instructions for defending grapes, and giving them the advantages of sunshine and the heat of a wall, &c.

of the bunch covered with glass: and thus I ripened fully several bunches as early as the middle of July, some even earlier, without any symptoms of scorching: taking care to let a due proportion of air flow under the glasses, yet not so little as I allowed to later fruit; by which management I was enabled to eat, from walls of the same aspect, ripe grapes all last summer, and from July to November, whilst many of my neighbours did not ripen any before the end of August or middle of September, and many others, from the old prejudice that grapes ripen best in the shade, neglected to remove the leaves until they lost the whole crop.

Another plan that I adopted, and which was most essentially useful, was carefully to nail down every branch to the wall, so as to make it come in close contact, as soon as they were of a size to make that operation possible.—And lastly. as soon as the bunches were ripe on the outer side, I had every one turned with the unripe side to the sun; to some even we gave a second turning, (an operation very easily performed with the hand when thus nailed close,) so that out of many hundred bunches of grapes of different sorts, among which was even the Gibraltar, I not only ripened completely every bunch, but I may with truth say nearly every grape on my wall, the greater part without glasses, after a certain period, but they were all forwarded by these glasses, removing them from one place to another as I gathered what they had completely matured.

I also found very useful at the latter end of the season, those small conical hand caps, which are made for about eighteen pence a piece, by running a rope with a tye from the finger hole at the point along the inner side, and there suspending them by a loop of cord to a nail on the wall; by which means they hang perpendicular with the wall's face, and are capable of covering and protecting from sluggs five or six bunches at once, brought together by altering the nailing. But this contrivance, which unites œconomy with utility, (for at this season they are out of use for any other purpose,) should only be applied when the grapes are advancing fast to maturity; as I have found by experience that the colour of the glass being green does not much advance their ripening, and that at the very latter part of the season it rather retards it.—For all my trials have taught me, that in proportion as the glass is white and transparent, nay even carefully cleaned, the grapes are benefited; and that next to the taking away the leaves from the walls which if left would prevent them from getting properly heated in the day time, the nailing the bunches themselves close to the wall is advantageous in the highest degree.

Useful instructions for defending grapes, and giving them the advantages of sunshine and the heat of a wall, &c.

Many of your readers (for I know on this business how much we have to contend with prejudice,) will think that my situation must have been remarkably suitable to the ripening of grapes, but I can assure them that it is not exactly the case, as my walls are almost within the city, much cut by the east wind, and have but little afternoon sun; but what will best satisfy them will be the confession that I could scarce at all ripen any on my Pergola or Espalier frames, and that even flasks would not ripen well some in that situation.

It is a serious thing to advise new management to old practitioners, but I can assure you that the success of this conduct, practised in the very teeth of their reprobation, was so complete, that I could have made wine of all my grapes, and that I actually dried some under these clear glasses nearly to raisins.—When all were full ripe I bagged in paper the remainder, and on Christmas day I eat my last bunches preserved by hanging them near the kitchen fire in the bags I cut them off in; while on the vines, in October, two bunches remained quite unripe that I purposely left at only two inches distant from the warmest part of the wall sheltered by leaves. What led me to this system of extreme exposure to light and heat, was the observation that I have often made both no the Rhine and in the neighbourhood of the Sabine Hills, that those who would have early grapes must even there give them plenty of sunshine: and on the Rhine we know that land for vineyards is exactly valued in the proportion in which it receives the sun, a mode of valuing land that in many cases I think will one day prevail every where, particularly on the sides of our poorer hills; for near Hochheim and the vineyards of Riefenstien, the highest and the lowest rented lands are often on the same hill, while here they seek the northern aspect to grow corn, forgetting the earlier products which the south can afford to the hand of industry, or rather

having

Useful instructions for defending grapes, and giving them the advantages of sunshine and the heat of a wall, &c.

having nothing more profitable to grow in their estimation; for it is only a very few years since, the walls and some of the vines now remaining, that a Mr. Fry had at Axbridge a well bearing vineyard on the southern side of that romantic hill, on land not worth at any rate more than half a crown an acre; and I have known a sack of very early potatoes sold for eighteen shillings, that were in a similar situation raised in the natural bed on land of no greater value; and filberts it is well known will grow on most of our southern poor hill lands, almost without the hand of culture.

Thus much I have thought might be useful to many people to know who have vines, which, for want of understanding these methods, they suffer either to remain as unfruitful ornaments, or coolly contemplate the destruction of, scarcely ever affording them the least manure, and expecting a spontaneous product once in six or seven years without any care at all. For although we have many expensive treatises on the management of vines under glass, except Evelyn in his 'French Gardener,' we have few authors who shew the possibility of raising a good English vineyard fit to make wine from; and as nothing is so easy as to make good wine from quite ripe grapes, I trust, by facilitating that operation, I shall render some useful service to the British wine grower, and, at any rate, increase the value of many vine-covered walls.

Great advantages of wool as a defence for peaches, &c.

And now, Sir, having taken up, I fear, but too much of your paper, I will only beg leave to add, as briefly as possible, that last year, for the first time, I used *coarse wool*, in the rough state, to screen my peach and apricot blossoms from the east winds, by tucking it into the east side of every bunch of bloom, instead of fern, laurel leaves, or broom; and that this afforded an effectual security to the fruit even after it was set; an improvement which has these advantages, that it is always at hand, is cheap, can be repeatedly used, gives no strokes to the wall in windy weather, and keeps up an even temperature in the night, while it makes less litter, gives less shade, and by being left on, encourages the growth of the fruit, by retaining the dews and securing the fruit stalk from the scorching reflection of the wall at noon.

Trusting that its utility may prove an apology for the haste with which the paper is written,

I remain,
Sir,
Always your obliged humble servant,
G. CUMBERLAND.

Useful Notes and Observations respecting the Islands of Orkney and Shetland. By Patrick Neill, *A.M. Secretary to the Natural History Society of Edinburgh.**

Norway abounds with trees. The Orkneys, &c. are bare.

THE circumstance of the shores of Norway being clothed with fir-trees†, is doubtless a strong analogical argument in favour of the practicability of raising timber in the Orkney and Shetland islands.

" In respect to the soil," (says the Bishop of Bergen‡), " it is not the good, rich and black earth, " that favours " the fir-trees; nor the clayey soil; but rather the gravelly, sandy, or moorish lands." This is an observation well calculated to inspire hopes of success.

Whether plants could be had from Norway.

Thousands of young fir-plants are cut, every spring, by the peasants of Norway, for food to their cattle. It would not probably be difficult, therefore, to procure quantities of saplings from that country. But if this were found to be too troublesome, it may be suggested that the ripe cones might be brought over (and these could easily be collected), and that the seeds might, by way of trial, be sown where the trees were intended to grow. This simple plan might possibly be found preferable to raising the plants in nurseries or gardens in the islands. We should, in such cases, adopt every approximation to the methods of nature. Pontopiddan even suggests, that instead of inserting the seeds in the soil, it would be better to hang the branches, containing the cones, upon poles at different distances, and to allow the seeds to drop out and sow themselves. At any rate, the seeds might be merely raked in. The experiment might be tried on any piece of dry rocky land (an acre or more), which could most easily be protected from the inroads of sheep or cattle, the exclusion of these being indispensable. The seeds might be sown very close; and if only one in ten or twenty were to vegetate, (and that is not a very sanguine expectation), a flattering foundation would be laid for ultimate success.

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* Extracted by permission from his " Tour through some of the Islands of Orkney and Shetland." The spirit of active industry and the consequent improvements in science, arts and manufactures in every part of our island, cannot be better shewn and promoted than by the travels of intelligent observers. Most of the subjects in the small book before us are of great national importance and interest, particularly at a moment when so many of our sources of prosperity are endangered.—N.

† The fir trees of Norway are, I find, the *fure*, or spruce, pinus abies (not the silver fir); and the *gran*, or pine, pinus sylvestris, well known by the name of Scots fir.

‡ Nat. Hist. of Norway, Vol. I. p. 143.

Remarkable fact that trees seem to prefer the west coasts.

Having mentioned this subject to Mr. James Hay at Gordon Castle, he observed to me, that "it is remarkable that trees thrive naturally on the *west* coast of Scotland, as well as on the *west* coast of Norway, in some places very nearly down to the sea side; while, in several places on the *east* coast of Scotland, they cannot be reared at all; and therefore whatever cause of difference may lie in the *soil**, it would appear that much is owing to *exposure*. The exposure to strong, sweeping unchecked winds, seems to be the chief obstacle to the raising of timber. Hills act upon the wind as a dam-dike does on a running stream, in producing considerable stillness or even calm upon the side from which the current flows. This consideration should induce planters to begin always at the bottom of hills, and extend their plantations gradually towards the sea. A hedge upon the side next the sea, though desirable, could scarcely perhaps be reared of any tree or plant. Hippophae rhamnoides (sea buckthorn) might be tried; but Sambucus nigra (elder bush) would probably be found preferable."

Larch, ash, sycamore, &c.

For the raising of larch, ash, sycamore, and others, nurseries should be established in the islands themselves; it being certain that plants resemble animals in becoming gradually habituated to particular climates and soils.

Salixes.

In places where Salix acuminata, S. arbuscula, aquatica, and others grow, various willows might be cultivated, suited for wicker-work and cooperage. Salix fragilis or crack-willow would grow freely; it makes large shoots every season, and bears cropping admirably. It answers well for making crets, cradels, and large baskets. The name *fragilis* only intimates that the annual shoot is very easily detached from the trunk, the twig itself being very flexible and tough. Salix viminalis or common osier, also grows very freely, and is much in request by coopers. Salix Helix, or rose willow; S. triandra, or long-leaved osier; and S. vitellina or yellow osier, would doubtless succeed, and they are all employed in basket-making. To these might be added S. Forbyana or basket osier, for the nicer kinds of work; and S. Russelliana, which would be very useful not only for making crets and creels, but in tanning, —the bark being superior for this purpose perhaps to oak-bark. A decoction of it would form an excellent liquor in which to steep their herring nets.

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* Is it not a general law over the face of the globe that the west sides of N. and S. chains, or mountainous ridges, are most steep?—N.

Molucca Beans.

On the Molucca beans cast ashore on Orkney.

I have lately observed a paper "on the beans cast ashore in Orkney," in Philosophical Transactions 1696, No. 222, by Sir Hans Sloane. He mentions three kinds as pretty common: the Cocoon: the Horse-eye-bean; and the Ash-coloured nickar. The two former are the kinds which I got in the islands, in 1804. The cocoon of Sloane is evidently the seed of the Mimosa scandens of Linnæus, the Gigalobium of Brown's "Jamaica." It is the largest of the beans figured in Wellace's "Description of Orkney," 1693. 2. The horse-eye-bean of Sloane is distinctly the seed of Dolichos urens Lin.; the Zoophtalmum of Brown, who calls the seed, ox-eye-bean. This is the smaller bean figured by Wallace, and is easily known by the hilus or welt which surrounds it, and which gives it somewhat the appearance of a horse's or ox's eye. 3. The ash-coloured nickar is the seed of the Guilandina bonduc Lin. It is not so commonly found as the others. It is a perfectly round hard seed, little larger than a musket-bullet.

Herring-Fishery.

Very great

This immense field for industry,—this inexhaustible source political advantages of the herring fishery of Scotland.

of wealth;—has been often described; but still it is in a great measure neglected; at least we certainly do not derive from it those vast advantages which it is calculated to afford, and which it did, for a very long series of years, afford to the States of Holland. At a moment when we are listening to the eloquent and plausible, but I fear seductive and dangerous arguments of the Earl of Selkirk in favour of emigration, I cannot omit this opportunity of very briefly calling into view the extent and the value of this fishery, which, if duly prosecuted, would afford cheerful and profitable employment at home, to any number of those deluded men who are every year abandoning their native country, in quest of imaginary happiness and riches in the woods and fens of America;—and I presume it will at once be conceded, that ten or twenty thousand Scotsmen engaged in the Shetland herring-fishery, would, in this eventful period, be a much more agreeable object of contemplation to the mother country, than the finest imaginable settlement in Prince Edward's Island, or on the banks of the St. Lawrence.

Immensity of the shoals.

It is scarcely possible to form an idea of the immensity of the grand northern shoal of herrings which approaches the Shetland Islands every month of June. "The flocks of sea-birds, for their number," it has been observed, "baffle the power of figures:"

> ———Where the Northern Ocean in vast whirls
> Boils round the naked melancholy isles
> Of farthest Thule;—
> Who can recount what transmigrations there
> Are annual made? what nations come and go?
> And how the living clouds on clouds arise?
> Infinite wings! till all the plume-dark air
> And rude resounding shore, are one wild cry*.

"But the swarms of fishes, as if engendered in the clouds, and showered down like the rain, are multiplied in an incomprehensible degree. Of all the various tribes of fishes, the Herring is the most numerous. Closely embodied in resplendent columns of many miles in length and breadth, and in depth from the surface to the bottom of the sea, the shoals of this tribe peacefully glide along, and, glittering like a huge reflected rainbow or aurora borealis, attract the eyes of all their attendant foes[*]."

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* Thomson.

—which is proved by the Dutch.

Let it not be thought that this swelling description exaggerates the amount of the shoals: let it be coolly considered that for more than a century the Dutch annually loaded above a thousand decked vessels out of this grand northern shoal, and yet that this immense capture never in any year sensibly diminished the number of herrings around Shetland, which, after these foreigners were glutted, regularly continued to press forward toward the islands in vast bodies, frequently crowding into every creek and bay!

Amount of the Dutch fishery.

The Dutch, it is well known, accounted this fishery their "gold mine." It seems generally agreed among authors, that it yielded them, for a long course of years, 3,000,000*l.* sterling yearly. Dr. Campbell, after premising that the value of the Dutch fishery has often been exaggerated, and that he will therefore give a "modest computation," proceeds thus: "It would however be no difficult thing to prove, to the satisfaction of the candid as well as critical inquirer, that, while it continued to flourish in their hand, they drew from their fishery out of the ocean washing the coast of Shetland, to the amount of two hundred millions sterling[*]." From 1500 to 2000 sloops were employed in fishing: this gave occasion to the freighting of 6000 more; and thus the herring-fishery gave employment and subsistence to above a hundred thousand persons[†].

Report of Capt. Smith in 1633 on this fishery.

Captain Smith, who was sent to Shetland so long ago as 1633, expressly to report on the Dutch fishery, says, "I was an eye-witness of the Hollanders' busses fishing for herrings on the coast of Shetland, not far from Ounst, one of the northernmost islands. Demanding the number of them, I was informed that the fleet consisted of 1500 sail, of 80 tons burden each, and about 20 armed ships, carrying 30 guns a piece, as convoy." The conclusion drawn by the captain, is quite characteristic of a British sailor: it is stated with much spirit, and though his plan is not a practicable one, his language forcibly shews how strongly his mind was impressed with the vastness of this fishery, and the absurdity of neglecting it: "If the King* would send out such a fleet of busses for the fishing-trade, being in our own seas, and on our own grounds, and all strangers were discharged from fishing in those seas, that the subjects of the three kingdoms only may have it, it would make our king rich and glorious, and the three kingdoms happy; not one would want bread,—and God would be praised,—and the King loved."

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[*] Bewick, Introd.

[†] Political Survey, Vol. I, p. 69F.

[‡] Ibid.

Former fishery at Shetland,

About half a century ago, the herring-fishery on the coast of Shetland was very successfully prosecuted by some English companies. But, through unaccountable mismanagement, it has for many years past been abandoned. At present, also, owing to the troublous state of the North of Europe, this fishery is more neglected by foreigners than at any period during the last two centuries. Very few Danes, Sweeds or Prussians, I understand, now make their appearance. The French and Dutch dare not. A few sloops from ports on the east coast of Scotland are scarcely worth mentioning.

—abandoned.

With respect to local position, the Shetlanders themselves are best situated for carrying on this fishery: but owing to poverty, the tenants or fishers are quite unable to engage in it: they can only take a few hundred barrels of the inferior kind of herrings which enter their voes in harvest. In summer 1804, a scarcity approaching to famine prevailed in Shetland; yet herrings, in countless myriads, were known to be off Unst. How deplorable to think that the people should starve while there was, at the same time, a "waste, at their doors, sufficient to feed half the human race!" The capital requisite for the purchase of sloops, nets, salt and casks, in order to an effectual prosecution of the fishery at sea, would, it is believed, exceed the ability even of most of the Shetland lairds.

Proposals for its renewal.

From Shetland, however, this fishery, if undertaken by English or Scots companies, could best be carried on. It would here be accompanied with least trouble and risk of delay, and with least expence. Shetland is near to the scene of the fishery: the Shetlanders are remarkably patient of fatigue in the fishing: they are accustomed to very sorry accommodation: and being habituated to indifferent fare, would not require that expensive victualling which is indispensable to an English crew.

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* Charles I.

The rules observed by the Dutch curers are now generally known*, and in some degree practised. But still it would probably be of considerable advantage if the influence of government were employed to encourage some fishing-famalies from Holland to settle in Shetland. A few Dutch curers thus dispersed among the British smacks, might prove exceedingly useful.

The markets are great and extensive.

May it not be hoped that some opulent English and Scottish companies,—under the fostering care of a paternal Government,—will undertake this Shetland fishery on a great scale,—a speculation which if persevered in, would surely, in the event, become exceedingly profitable. The Hamburgh market alone would take off the produce of a hundred sloops, except the taste for Shetland herrings has declined in the north of Germany. There is a great demand for herrings from our West India colonies, for the food of Negroes; and the home consumption would surely not be inconsiderable. If every inhabitant of the island were to eat only two herrings in the year, it would open a market for the produce of another hundred sloops, even supposing them to fish with the greatest possible success. The herring fishery is an undertaking, indeed, of national importance, not merely as a source of wealth, but as an additional nursery for our navy.

If this fishery were to be extensively carried on from Shetland, some additional villages would become necessary, and winter-employments would be wanted. The manufacture of herring-nets might properly and advantageously occupy many during the winter: and with this, might commodiously be joined the manufacture of lines for the cod and ling fishery.

Particulars by Dr. Halliday respecting the herring.

To these very cursory and imperfect hints on the importance of this fishery, I shall subjoin a few remarks connected with the natural history of the herring, for the principal part of which I am indebted to my friend Dr. Halliday of Edingburgh, (now Halesworth in Suffolk).

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* They are printed in the Transactions of the Highland Society of Edinburgh, Vol. II. 328—345.

I am aware that Dr. Anderson, in his Agricultural Recreations, has rendered it highly probable that the herrings, instead of rendezvousing near to the North Pole, as was formerly imagined, only retire a little way from our coasts, or sink deeper in the sea, at particular seasons. He remarks, that the fishery commences sooner in some southern bays, than in others that are more northerly: that the return of the grand shoal to the northward is never observed: that from peculiarities in the shape and size of the herrings at different fisheries, it is evident that the herrings of the same breed, or partial shoal, return annually to the same shores: and, that they do not retire towards the North Pole to spawn, as was formerly imagined; but on the contrary, are taken on our coast, both when full of roe, and immediately after spawning, when the fry are seen.

They breed near the coasts,

This last observation of the Doctor's is undoubtedly correct. The fry is, at particular seasons, seen in the Orkney and Shetland seas in incredible numbers: it is then called the herring-soil, and is accompanied by thousands of the smaller gulls and divers.

—and grow very fast.

The growth of the fry is very rapid; it has been watched by Dr. Halliday, who informs me, "that on the western shores of the Isle of Mull, he has observed, in the months of March and April, the herring-spawn which was accidentally entangled by the cod-lines, to be vivified; the two eyes and head of the herring being then discernible; and that this spawn was raised by those lines only, which were set on the banks at some distance from the shore. In a fortnight, however, he observed the fry, about an inch in length, in great swarms close by the shore; and in six weeks they were three inches long."—Hence Dr. Hallidy concludes, that it is possible the herring may attain its full growth in one year, instead of requiring three, as Dr. Walker and others have supposed.

They do not go very far from land.

Dr. Halliday further informs me, that he has observed that the herrings leave the western shores of Mull when about six weeks old, and steer to the northward: but that they do not go many leagues from land, he considers as beyond doubt. He conceives that some place not far distant from the island of Unst may be their rendezvous or grazing-ground, (if we may be allowed the expression):—that during the harvest and winter they keep near the bottom, where they feed and grow to maturity: that in the spring they collect, rise to the surface, and begin to move off in various directions to the southward, for the purpose of spawning.

—but deposit their spawn near the middle of bays.

As already remarked, they do not deposit their spawn near the shore, but in the middle of the lochs or bays, or on the banks which are generally to be found at the mouths of the lochs. If, however, they are frightened from the spawning ground, they fly towards the shores, and are then full of roe; but they soon retire again, and do not return till freed from their load. They then range along the shores for some time, and at last retire towards the north, following the fry of the former years.

It may be proper to add, that it is frequently observed on the western coast of Scotland, that a few weeks after the first shoal has left the lochs, a second shoal enters them, in full roe. This second shoal appears in the end of October or beginning of November: they deposit their spawn and leave the lochs as before. It is possible that the fry which leaves the coast in the beginning of May, may be the same that returns to it next year about the same period, and that these may proceed from the spawn deposited in the latter end of the season; while the fry of the June spawn having got off before the winter commenced, may return the following November;—thus allowing, from the depositing of the roe, to the maturity of the herring, eighteen months.

XIII.

Description of a very useful Bolt for Bookcase Doors. By Mr. Peter Herbert, *No.* 33, *Bow Street, Covent Garden**.

Properties of the bolt.

Mr. HERBERT presented to the Society a model of his invention. He intended it for a library book-case bolt, to facilitate the opening of both doors at once, and to secure the same, without the trouble of bolting two bolts in the common way. It will do for wardrobes, French casements, or folding sash doors, and will also make a good sash fastening, if let into the bottom sash, with a small brass knob to slide as common; it would bolt in the frame by the side of the sash cord, both sides at once; and he can also make it answer sundry other useful purposes if required.

Reference to the Engraving of Mr. Peter Herbert's Bookcase Bolt, Plate IV. Fig. 2.

Description.

K, L, Fig. 3, represents the two stiles of the doors of a folding bookcase.

M, the key-hole of a lock with two bolts, which are more clearly shewn at Fig. 3, where the back of the lock N shews the two bolts of the lock pressing back a sliding piece, O; on the front part of this sliding piece in Fig. 2, two small friction rollers are placed at P, in the act of pressing against two levers, crossing on one common fulcrum R, to each end of which shorter levers, S S, above and below are connected by joints. These short levers act upon two long bolts, whose extremities are shewn at T T, having each a helical spring at V, V. In the state as engraved, the doors are locked and bolted.

On drawing back the bolts of the lock by means of the key, the helical springs V V press against the plates U U, through which the long bolts pass; they force back the long bolts and sliding piece O, and allow both the doors to open.

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* Soc. Art. 1806. Ten guineas were given for this improvement.

XIV.

Description of an improved Door Latch. By Mr. JOHN ANTIS[*].

SIR,

I DO not doubt but that you are persuaded of the necessity of having a door-latch superior to, and not so liable to be out of order as those hitherto in use, in the door-locks of dining-rooms, &c. Some time ago, I made an attempt to contrive such a one, which I fixed into a small box by itself; I have now tried it for some years in my own house, during which time I never found occasion to clean or to oil it. I at that time thought there would be a difficulty to introduce it into a mortise lock, in such a manner as to place the knobs and the key-hole symmetrically. That difficulty I have now overcome, and take the liberty to send you a pattern for your inspection.

Imperfections of the door latches now in use.

My object has been to contrive a simple latch, as much as possible without friction, not more expensive than those hitherto in use, and capable of moving smoothly and easily without the necessity of cleaning and oiling, as long as the metal will last of which it is made. How far I have succeeded, I leave to the decision of the Society.

Characters of the improvement.

I am, Sir,
Your humble servant,
JOHN ANTIS.

Fulneck, April 3*d*, 1804.
To CHARLES TAYLOR, ESQ.

Reference to Plate IV. Fig. I.

A, shews the hole for the handle, which moves the follower and latch. B, the follower which draws back the latch, on turning the handle either way. C, the latch. D, the longitudinal spring, which throws out the catch of the latch when the hand is withdrawn. E, the small bolt, to secure the door internally. F, the key-hole, the bolt of the lock of which is not shewn, being placed above the key-hole.

Description.

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[*] Soc. Arts. This useful contrivance was rewarded with the silver medal.

SCIENTIFIC NEWS.

USEFUL NOTICES RESPECTING VARIOUS OBJECTS.

1. *Method of preventing Wet from being introduced into Rooms by Windows which shut together like folding Doors.*

Easy remedy to prevent rain being driven into apartments through the interstices of window frames.

A considerable inconvenience has been found from the wet penetrating, in rainy and windy weather, through the joints of those windows which have been called French windows, and are now much used. No accuracy of workmanship has been sufficient to remedy this evil; but, on the contrary, the closest joints have seemed rather more favourable to this effect than others less neatly made. Mr. Collinge, Engine-maker, of Lambeth Road, shewed me a very simple and easy remedy. Reasoning on the subject, he considered the close joint as a capillary interstice which would retain a continuous mass of water, much more disposed to be driven horizontally into the room by the action of the external air than to be conveyed downwards through a longer interval by its mere gravity. He has therefore enlarged the space for descending water by ploughing out a semi-cylindrical groove in each concave angle, from top to bottom. This small space, which is about one-tenth of an inch wide, occasions no deformity, and allows the water, as soon as it arrives there, to trickle down to the bottom of the frame, where it is conducted off by a similar concavity along the horizontal frame-work to any place of external discharge which may be made choice of. This easy and effectual cure for a nuisance which has destroyed the carpets, and occasioned puddles in very elegant rooms, and has apparently resisted all efforts to remedy it by close fitting, will, no doubt, be acceptable to many readers.

2. *Extemporaneous Printing Press, used by Country Comedians.*

Simple method of letter-press by means of a roller.

I was informed, the other day, that it is the common practice of travelling companies of comedians to print their bills by laying the damped paper upon the form of letter previously inked, and to give the pressure by a wooden roller, clothed with woollen cloth. Many years ago I made experiments of this method, which I found very capable of affording impressions, by a light pressure. The form of letter must be disposed in a kind of frame, having its upper surface about one-thirtieth of an inch lower than the inked face, in order that the roller, being supported by the frame, may not be obliged to rise with much obliquity, upon the first letters; and that it may pass off, at the other end, with equal ease. If some such contrivance were not used, the paper would be cut, and the impression injured at the beginning and end of the rolling. The roller must be passed in the direction of the lines, or across the page; otherwise the paper will bag a little between line and line, and the impression will be less neat. In fact, the common method by the plattin, or flat surface which presses the whole at once, is best; but the engine is less simple.

But as the arts of writing and of printing have incalculably extended the knowledge and powers of man, it may be allowed us to look forward to a time when communications shall be as much more rapid and effectual, compared with those of the present time, as ours are, compared with what they were before printing was invented. We may hope for a time when men shall confer more rapidly, concisely, perspicuously, and comprehensively by writing than they are now able to do themselves by articulated sounds. We may contemplate a period when by easy combinations of chemical and mechanical skill, the multiplication of numerous copies may demand scarcely more time and apparatus than is now required to write a single copy. And while we speculate on possibilities of this nature, which are far from being in the higher class of improbabilities, we may indulge a philanthropic hope, that when it shall be more easy to convey, distribute, and apprehend the results of philosophical and moral research, the short span of human life will be much less obscured by misery and accumulated suffering than it is at present. Every step toward these ends is surely entitled to our notice.

3. *Art of Printing from Designs made upon the Surface of Stone.*

Prints or impressions from drawings upon stone.

I am not at present in possession of the history of an art which has been practised for some years in this town by several ingenious foreigners; namely, that of printing copies from designs made on the surface of stone. An eminent chemist informs me that the method is as follows:

Upon the surface of an hone, or close grained stone, designs are to be made in the stroke manner, with a pen, by means of an ink or pigment, made of a solution of lac in leys of pure soda, with a little soap added, coloured with lamp-black; or the designs may be made with a crayon of the same composition. I suppose that the proportions and manipulation would require some trials before perfect success would be obtained. When the design has been allowed to dry or harden for three or four days, the stone may be soaked in water, and its surface wetted. In this state if it be dabbed with Printers' ink from the balls, the ink will stick to the design, but not to the naked stone, and a copy may be taken from it by applying wet paper with pressure; whether of a rolling or screw press was not mentioned, but I suppose the latter to be preferable.

The advantage of this art appears to be that the print is given from an original, and not from a copy, as all engravings must necessarily be. It may also be considered as one of the means adverted to in our last article. For if a smooth stone, or a board of close wood, or perhaps some species of tile, or other prepared surface, could be written upon by an ink which, when speedily dried by the fire, or otherwise put into a state fit for use, could be made to afford impressions or copies by a simple roller, it would be easy to multiply bills, orders, notices, and an infinite number of other useful papers, to an extent which cannot at present be developed without much investigation and research.

3. *Gilding by means of Zinc.*

New process of gilding.

The same intelligent and active philosopher, whose name I forbear to mention only because I have not at this instant an opportunity of asking his permission, informs me that a coating of brass, formed by the precipitation of zinc upon copper, constitutes the surface of the beautiful gilt trinkets which at present abound in our shops, and are much superior in their appearance, and cheaper in price, than what were formerly made.

The process is, Take of zinc one part and mercury twelve parts, with which make a smooth soft amalgam. It is better if a little gold be added. Clean the copper piece, or trinket, very carefully with nitric acid. Put the amalgam into muriatic acid, and add argol (by which name the crude tartar is denoted in the shops). Purified tartar will not do. Boil the clean copper in this, and it will be very finely gilt. Copper wire, thus coated, is capable of being drawn out to the fineness of an hair, though copper alone would not. This wire is used for making gold lace, and for epaulets and other similar articles.

The theory of the above process appears to resemble that of whitening pins; and its useful applications may probably be more numerous than those which have yet been adopted.

4. *Clock of the famous John Harrison, which does not require cleaning.*

Harrison's clock without oil.

Cummings, in his Treatise on Clock and Watch Work, mentions a clock of Harrison's which was constructed to go altogether without oil; but he does not say by what means the necessary lubricity of its moving parts was obtained. About two years ago I saw this clock in the hands of Mr. John Haley, Jun. The pivots of the wheels moved on friction rollers of considerable diameter; and the pivots of these rollers, or rather wheels, were brass, and moved in sockets of a dark coloured wood, which I think must have been lignum vitæ. Hence it should seem that the contrivance was reduced to that of rendering the surfaces of contact, where the sliding or friction was to take place, as slowly moving as possible, and in presenting a face which should afford a softish bed, having grease in its interstices. Similar to this is the practice of some mechanics, who make the bearing parts of the axis of a grindstone very smooth and round, and envelope them with a piece of bacon-skin, which is said to be very useful to keep away the sandy particles, and facilitate the motion for a long time without much wear.

TO CORRESPONDENTS.

Extreme occupation during the concluding month of the year has prevented my searching into the authorities upon which De Lalande has established his comparison of the English and French measures, and also those from which he has deduced the measures of the earth's radii. I shall pay attention to the request of "A Constant Reader" in the next Number.

Mr. Walker's letter from Oxford arrived by the post; but not the pamphlet.

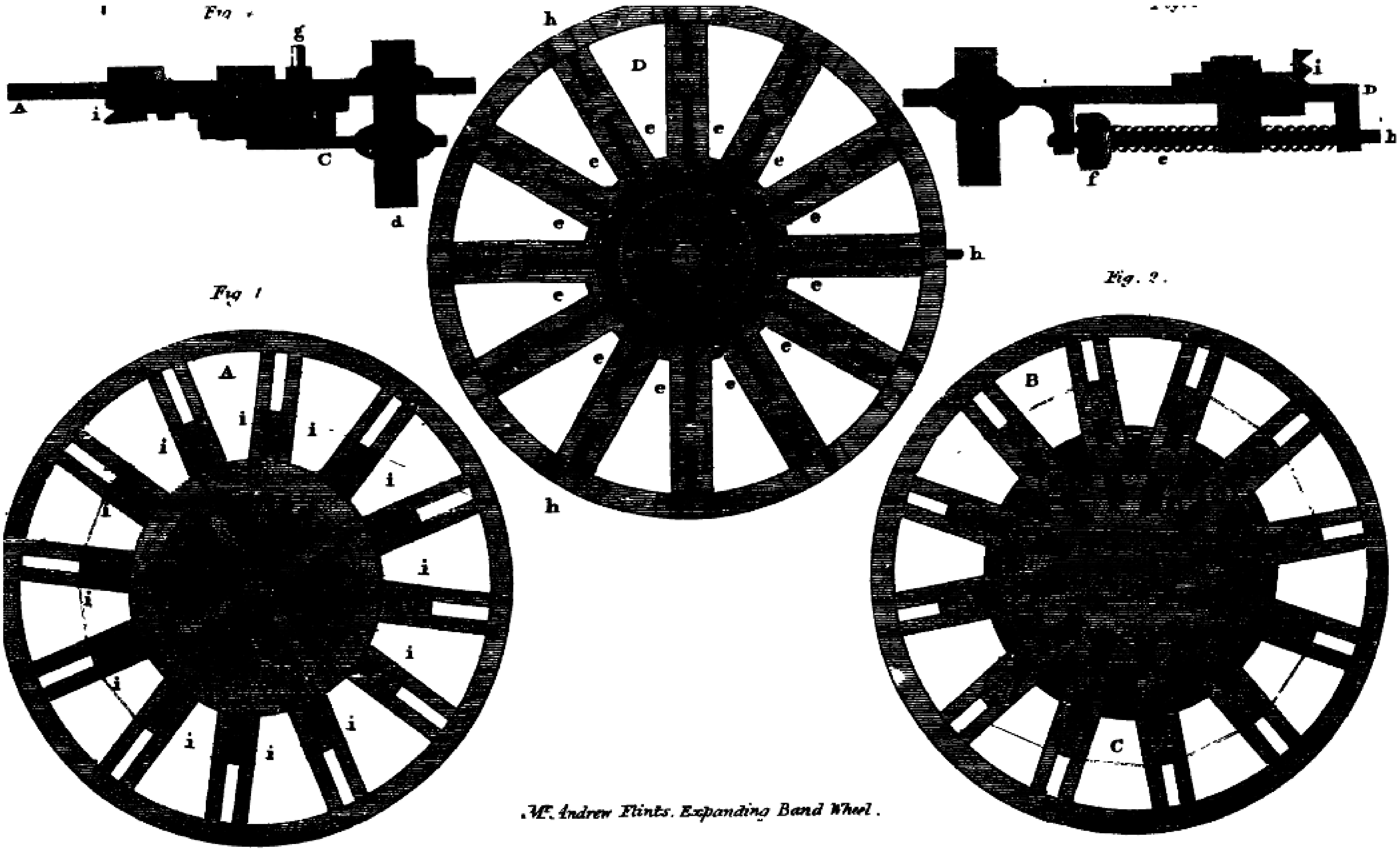
In answer to the inquiry of D. M. respecting a method of cleansing linen by the application of steam, as used by the French, I cannot point to any authentic account of a simple process of this kind, though I have been informed that the application of steam to piece goods, in a large digester, at a temperature considerably above 212°, is very effectual in cleansing, and promoting the bleaching process. This, however, seems fitter for the manufactory than the laundry. I am disposed to think that the method alluded to by D. M. is the Salzburg method, described in Van Mons's Journal, of which a translation is given at p. 127 of the tenth volume of our Journal, containing particular instructions how to carry it into effect.

I am sorry that a note of R. L. Edgworth, Esq. was not noticed earlier. Four lines from the bottom of page 82 of the last volume, the following should be inserted: "The number of teeth necessary for the wheel may be easily calculated to suit the measurement; so that the dial-plate may shew with sufficient accuracy five, or any other small number of miles."

Mr. R. L. E. speaks with commendation of Mr. Gilpin's crane in that volume; but remarks, that the groove which renders a common chain so much preferable to a rope for heavy burdens supported by tackles, has been long used.

I have just received the work of the Rev. P. Roberts, A.B.

Dr. Bardsley, Physician to the Manchester Infirmary, has committed to the press a Selection of the Reports of Cases, Observations, and Experiments, chiefly derived from Hospital Practice; including, among others, Clinical Histories of Diabetes (with Chemical Experiments on the Nature of diabetic Urine), Chronic Rheumatism, and Hydrophobia.



Mr. Andrew Flints. Expanding Band Wheel.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY

AND

THE ARTS.

MARCH, 1807.

ARTICLE I.

Experiments on Palm-Oil, by John Bostock, *M.D. Communicated by the Author*

To Mr. NICHOLSON.

Palm-oil,

THE appearance and physical properties of the substance called Palm-Oil, are sufficiently well known; but I believe its habitudes with different chemical re-agents, have never yet been attended to.

—its obvious properties.

Palm-Oil, as usually imported into this country, is of a deep orange-colour: its consistence is similar to that of butter, although perhaps, for the most part, a little harder and less unctuous. It has an odour peculiar to itself, somewhat aromatic, and not unpleasant. Its inflammability seems about equal to that of tallow; a cotton thread, inclosed in a quantity of it, was easily ignited, and burned with a clear, bright flame.

Experiments respecting its melting point.

In order to ascertain the melting point of palm-oil, I heated a portion of it to the 100th degree, when it became perfectly fluid, and then observed the effect produced on the thermometer by its gradual cooling. When the mercury had descended to the 69th degree, the oil began to be slightly opake;

at 62°, it was completely so, and was of the consistence of honey: it continued to grow thicker until it arrived at 45°, the temperature of the room, when, although its fluidity was entirely lost, it still retained a degree of softness that it did not possess before the experiment.

The thermometer, as far as I could perceive, continued to descend without interruption during the whole period, and the oil seemed gradually to thicken in every part, without exhibiting any appearance of partial congelation. The inference which may be drawn from this experiment, seems to be confirmed by the following: Two equal quantities of the palm-oil were placed in similar jars; one portion was rendered completely fluid, and was then cooled down to 69°, when it began to assume a slight appearance of opacity; the other was heated to 65°, and was just beginning to melt. Both vessels were then plunged in a water-but of 100°: a thermometer inserted into each of them rose with equal rapidity, the first remaining 4° above the second. They were then removed, and the thermometers indicated an equally rapid decrease of heat, until they arrived at 48°, which was the temperature of the room. Equal quantities of palm and olive oil were heated, in similar jars, to the 100th degree, and then removed to a temperature of 45°: thermometers were inserted into each, and descended with equal rapidity.

Habitudes of Palm-oil with Alcohol,

Alcohol, at the ordinary temperature of the atmosphere, acts upon palm-oil in a very slight degree only. After remaining in contact for forty-eight hours, the fluid is perceptibly tinged of a yellow colour; and, by the addition of water, a slight degree of turbidness is produced, owing to the precipitation of a small quantity of palm-oil. By the application of heat, alcohol dissolves the oil more readily; a part of it is precipitated as the fluid cools, but a small quantity, about 1-75th of the weight of the alcohol, remains in permanent solution, and may be precipitated by water.

—and with sulphuric ether.

Sulphuric ether acts upon palm-oil with facility, at the ordinary temperature of the atmosphere, and produces a deep, bright yellow solution. The ether dissolves about 1-6th of its weight of the oil, and its solvent power is increased by heat. When water was added, the ethereal solution rose to the surface, and floated on the water without being decomposed.

Palm-oil is also readily dissolved by the oil of turpentine, at the temperature of the atmosphere.

Palm-oil has less attraction for pot-ash than olive-oil.

The action of caustic pot-ash upon palm-oil is similar to that which takes place between the alkalies and other bodies of an oleaginous nature. After being boiled together for some time, they form an opake and semifluid mass, miscible with water without decomposition, but which is slowly decomposed by the addition of an acid. In this latter case, the oil rises to the surface in small flushes, having lost its original colour and smell. The same effect, although in a less degree is produced by the action of ammoniac upon palm-oil. Palm-oil, however, exhibits less affinity for the alkalies than olive-oil.

Action of the sulphuric and nitric acids.

Palm oil does not appear to be soluble in mineral acids. After being heated for some time in contact with them, it was left floating on the surface of the fluid and, upon saturating the acids with an alkali, no precipitation was produced. The oil had, however, undergone a considerable change in its appearance and properties, from the operation of the sulphuric and nitric acids. In the former case, it had lost its specific smell; it was of a grey colour; and was considerably less unctuous than before the experiment. Upon being immersed in boiling water, it appeared to consist of two substances, of a white friable matter, which was diffused through the water, and had partly lost its oleaginous nature, and some small drops of a blackish oil. The effect produced by the sulphuric acid seemed to be similar to that which is described by Mr. Hatchett, in his valuable papers on the production of tanning*.

Oxidation by nitric acid.

The oil that had been heated in contact with nitric acid was also considerably changed: it was of a dirty colour, of a much firmer texture than in its natural state, and had acquired a smell resembling that of melted wax. The appearance of this substance seeming to coincide with the prevailing theory respecting the oxidation of oil, I was induced to examine how far it resembled wax in its chemical properties. First, in order to ascertain its melting point, a quantity of it was completely fused at a temperature of 110°. It was then gradually cooled; and when it had arrived at the 72d degree, it began to grow opake at the edges; at the 69th degree it had entirely lost its transparency;

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* Phil. Trans. 1805, p. 11, and alibi.

transparency; and, at 65°, it was become so firm, that the thermometer could with difficulty be removed from it. Hence it appears that palm-oil, by the action of nitric acid, is rendered less fusible, and that its fusibility is more nearly confined to a precise limit than in its natural state. Its solubility in alcohol appeared, however, to be rather increased; 100 grs. of alcohol dissolving very nearly 3 of the oil, two thirds of which were precipitated as the fluid cooled. The tendency of the palm-oil to unite with pot-ash was also considerably increased by the action of nitric acid. Equal quantities of the oxidated oil, and of the palm-oil in its natural state, were boiled with twice their weight of liquid pot-ash, nearly the whole of the oxidated oil was united to the pot-ash, and formed with it a thick saponaceous substance, while a considerable portion of the common palm-oil remained floating at the surface.

Effects of nitric acid on palm-oil continued.

Nearly the same effect was produced upon the palm-oil, by being boiled with nitric acid, by being digested in it for some weeks, at the temperature of the atmosphere, or by being precipitated, by the nitric acid, from its union with pot-ash. When the oil was digested without heat in the acid, its colour was first changed to a dirty green, next to a grey, and, lastly, was rendered nearly white. That, in these different processes, the oil was not united to the entire acid, but that a portion of the acid was decomposed, and its oxygene absorbed, I judged, because I found that the oil, after it had undergone the change, was not in any respect altered by being kept for some time in boiling water, nor did it impart to the water the least degree of acidity. This opinion was farther confirmed, by its union with pot-ash; if the oil had contained nitric acid, the addition of the pot-ash, instead of forming soap, would have reduced the oil to its original state.

Comparison with other oils

After having ascertained some of the leading properties of palm-oil, it appeared an interesting object of inquiry, to examine the relation that it bears to other substances, both of animal and vegetable origin, to which it exhibits some points of resemblance. I particularly refer to the expressed oil of vegetables, butter, tallow, spermaceti, the wax of the myrica cerifera, bees-wax, and the resin. The properties to which I particularly directed my attention, were the fusibility of the substances, and their habitudes with alcohol. The melting points of several of them, I had, on a former occasion*, taken some pains to ascertain with accuracy, on account of their having been so differently stated by authors of the first respectability. I now repeated the experiments with every possible care, and obtained the following results:

Freezing-points of various oily substances determined.

Tallow, heated to 120°, was perfectly fluid and transparent; at 99¾°, a slight tendency to opacity was just perceptible; at 97°, it became very evidently opake round the edges; and at 90°, it was no longer transparent; at 89°, it had acquired a pretty firm consistence. The thermometer continued to descend during the process without any apparent interruption. A quantity of spermaceti was heated to the 120th degree, when it was perfectly fluid and transparent. The mercury descended to the 114th degree, when a slight opacity was perceptible at the lower edge; but it continued falling to 112¼°, when it became stationary. A film then formed on the surface, and very nearly the whole was rendered solid, when the thermometer began to descend again; but, upon agitating the part that remained fluid, the mercury rose to 112¼°. When the whole had concreted, the thermometer descended to the temperature of the room. Upon going through a similar process with myrtle-wax, heated to 120°, the opacity was observed to commence at the 116th degree; but the mercury did not become stationary until it arrived at 109¼°: here it stopped until the whole became solid, when the thermometer again began to descend. Bleached bees-wax showed a slight degree of opacity at 148°; but 142° or 141¾° was the point where the mercury became stationary. The wax, however, retained a degree of softness at a much lower temperature.

Palm-oil melts at a low heat, &c.

With respect to their fusibility, these bodies will stand in the following order—expressed oil, butter, palm-oil, tallow, myrtle-wax, spermaceti and bees-wax. I had not an opportunity of making the experiment upon expressed oil; but butter, palm-oil, and tallow are not only more fusible than the other substances, but they also agree in being liquified in a gradual manner; whereas the others pass more immediately from the fluid to the solid state, at one precise degree of temperature. With respect to the effects of alcohol, it is an opinion universally received, that expressed oil, butter, and tallow are not acted

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* Nicholson's Journal, IV. 193 and seq.

Action of alcohol on expressed oils.

upon by it. This opinion, however, I found erroneous; not only was a small portion of each of them dissolved by being heated with alcohol, but even without the assistance of heat, a minute, yet very evident quantity, was taken up by the spirit. A part of the substance dissolved in the heated alcohol was precipitated as the fluid cooled, the remainder was separated by water, or by evaporation. The quantity was so small, that I found it difficult to ascertain its exact proportion.

Method of making the experiment.

The method that I pursued with respect to the spermaceti and the other kinds of wax, was to add them by degrees to the boiling alcohol, until a quantity remained undissolved. This would necessarily be melted, and would form itself into a small globule, which, when the fluid was become cool, might be removed. The fluid, together with that part of its contents which was precipitated by cooling, were then thrown upon a filtre, the weight of which was previously known, and the precipitated part being retained by it, it was easy to ascertain its amount. By weighing the fluid that passed through the filtre, and by permitting the alcohol to evaporate spontaneously, the solid contents that had been dissolved in it were ascertained. In this way were discovered both the whole quantity of the body that the alcohol dissolved, and that part of it which was continued in solution after the fluid had cooled.

Results of the action of spirit upon fat substances.

Proceeding in this manner, I found that 100 grs. of alcohol dissolved 52 grs. of spermaceti, half of which precipitated by cooling: 100 grs. of alcohol dissolved 2.134 grs. of myrtle-wax, 1.334 grs. being precipitated by cooling, and 1-8th gr. held in permanent solution. The same quantity of alcohol dissolved only .31 gr. of bees-wax, almost half of which was precipitated. The order in which these substances will stand, according to their power of resisting the action of alcohol, will be, olive-oil, butter, and tallow, nearly the same, bees-wax spermaceti, palm-oil, and myrtle-wax. The order of fusibility is, therefore, not exactly the inverse of the order of solubility in alcohol.

Their attraction for alkalies

The affinity of these several substances for the alkalies nearly follows the order of their fusibility, although not exactly so, tallow appearing to unite with caustic pot-ash more readily than with palm-oil.

Habitudes of Resins.

With respect to the resins, their fusibility and their solubility in alcohol, differ considerably in the different species; in general,

neral, however, they are less fusible, and more soluble in alcohol than any of the bodies mentioned above. It appears then, upon the whole, that palm-oil differs essentially in its physical and chemical properties from any substance that has hitherto been made the subject of experiment. Its fusibility is nearly similar to that of animal fat, while, in its chemical properties, it more nearly resembles the resins, at the same time that it differs from those bodies in not being soluble in nitric acid.

Liverpool, Feb. 14, 1807.

II.

Description and Use of a Calorimeter or Apparatus for determining the Degree of Heat, as well as the Economy attending the Use of various kinds of Fuel. By M. MONTGOLFIER.

Advantages of economy, &c. in fuel.

THE proper use of fuel is one of the most important objects in all the processes of the Arts, and more especially in Chemical Operations; and it is an object of no less utility, to determine the advantage and economy attending the uses of the various descriptions of fuel and the intensity of heat disengaged from the substances burned.

Different combustibles vary in their effects.

The same quantity of combustible matter of different kinds does not always afford the same degree of heat, and a longer or shorter portion of time will be required to disengage it from each combustible respectively. The success of an operation frequently depends on the rapidity with which it can be performed. Manufacturers, distillers, and cultivators must therefore consider it as an object of great importance to know what kind of fuel may be the cheapest for use, and what may be the proportion of a given quantity of the one compared with the same quantity of another, with regard to the effect to be derived from each; or, in short, what may be the most certain and easy method of determining the difference of the action of heat. The editors of the *Journal des Mines* speak with approbation of Mr. Montgolfier, for the instrument of which they have given a description, at the same time that they remark, that it very essentially differs from the instrument invented many years ago by Lavoisier and La Place.

Description of the Calorimeter.

The Calorimeter described.

Plate 5, exhibits a section of the Calorimeter of Montgolfier. A B C D is a vessel or box of tin, which might, with more economy and advantage be made of wood, sufficiently well constructed to hold water. In its cover A B, there is an opening *a b*; and so likewise, in the bottom, is an opening *e f*. Within this vessel is a small stove, *a b c d e f*, of plate-iron, or, which is better, of copper, carefully closed, so that no water can enter into it Its lower opening corresponds with that of the exterior vessel or box, *e f*. The other opening, in the other part is closed near *a b*, by a stopper which can be taken out at pleasure.

—It consists of a vessel having a stove within it, and a descending flue or chimney.

c d is a grate composed of iron wire, upon which the fuel is put, the ashes fall through the grate, and escape at the opening *g*.

Near *h i* is fitted a tube, *k k*, through which the smoke escapes by the opening *l*. This pipe must be made of iron or copper plate, sufficiently close to prevent the water from penetrating. *m m* is a pipe of plate iron, surrounding the last-mentioned in such a manner as that the water may be placed in the place between them. E is a reservoir, of which the cover, *r s*, can be taken off, in order to fill the apparatus with water.

o o is a pipe proceeding from the same reservoir, and communicating with the pipe *m m*.

n n is another pipe, which passes from *m m* into the vessel, for the purpose of introducing water, after it has passed through the pipe *m m*.

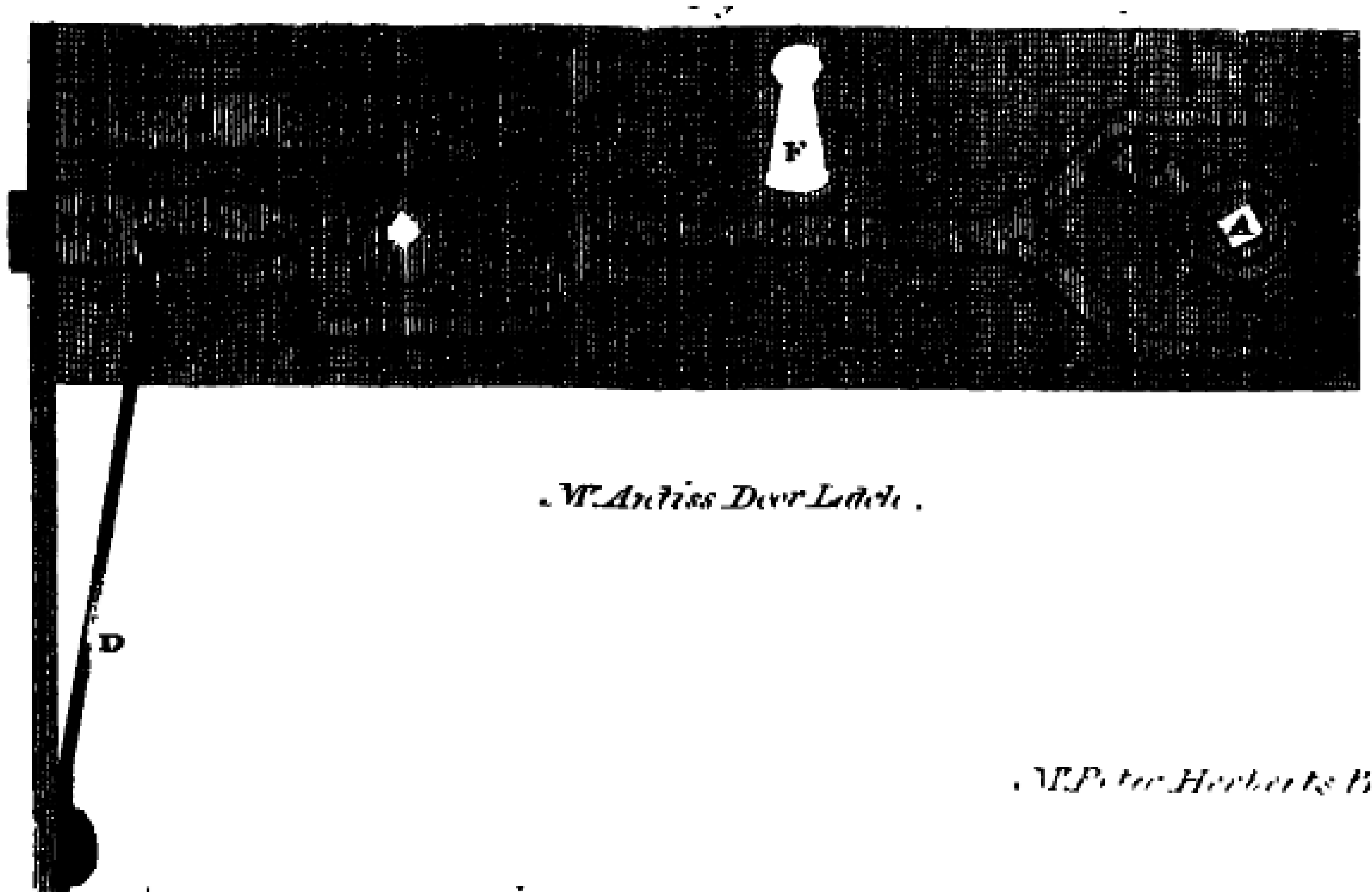
p is a cock, through which boiling-water may be suffered to escape; and *q* is another cock, by means of which the apparatus may be emptied when needful.

F G are the legs which support the apparatus.

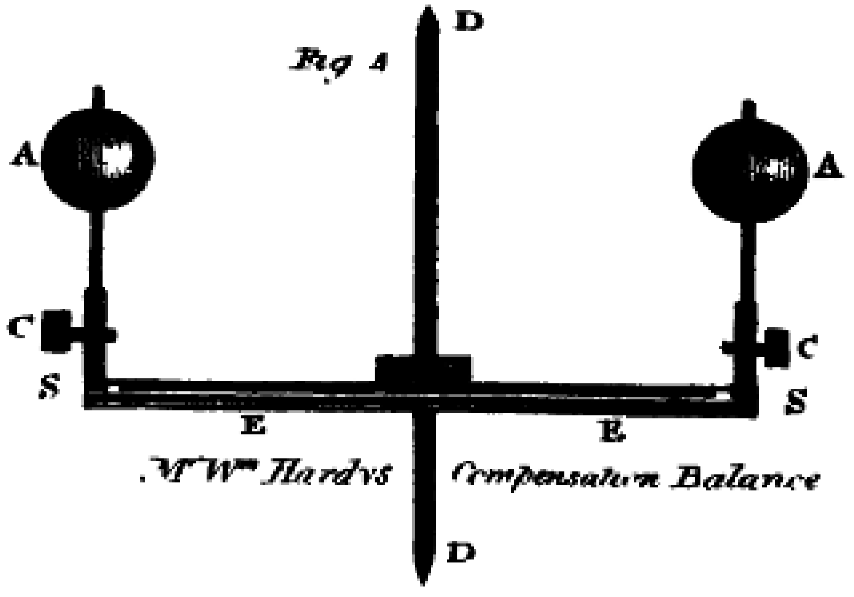
Use of the Calorimeter.

Method of using it.

When it is required to determine the time in which different combustibles disengage, an equal quantity of heat, the reservoir *e* is to be filled with water. The fluid passes through the tube *o o*, rises through *m m*, and thence, by *n n*, into the vessel A B C D. A sufficient quantity must be poured to fill the

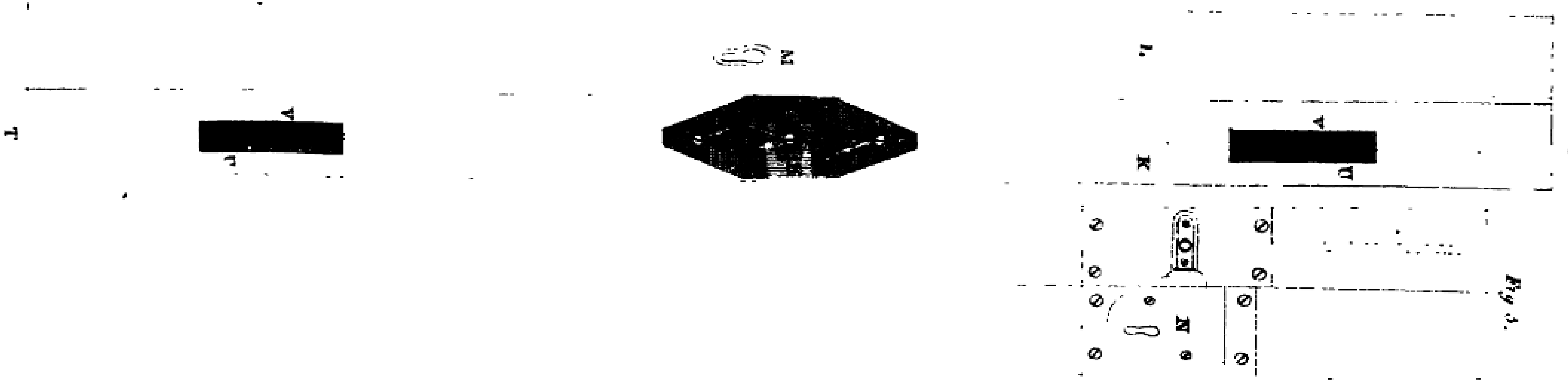


Mr Antiss Door Latch.

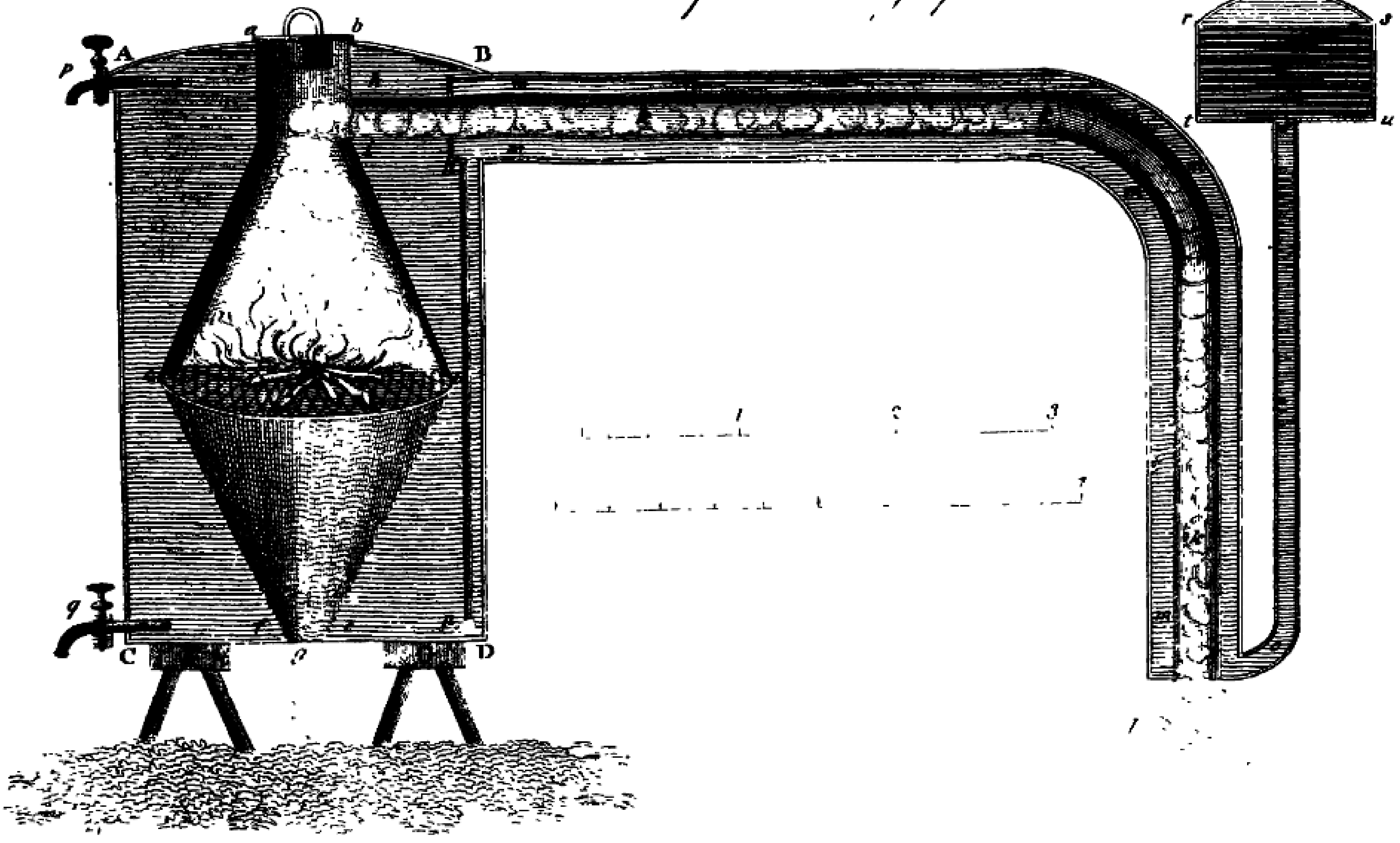


Mr Wm Hardys Compensation Balance

Mr Peter Herberts Book Case Bolt



Calorimeter by M. Montgolfier.



the whole internal capacity of the vessel, which is easily known when the water does not descend below the line *t u*, or the most elevated station of that fluid in the apparatus: and the temperature must then be noted by a thermometer. A sufficient quantity of the fuel, for the purpose of an experiment, must then be taken; for example, wood cut into small pieces, and placed in the grate *c d*. After setting fire to it, the upper opening *a b*, of the stove, is to be closed, and notice taken of the time employed in raising the water to a certain heat; for example, that of boiling, which may be ascertained by a thermometer. At this period the fire is to be taken out, and the water and the apparatus suffered to cool to the first temperature at which the operation commenced. Another kind of fuel; for example, pit-coal or turf is then to be disposed on the grate *c d*, and the same observation made, after setting it on fire.

By observation of the time required to heat the water, the effects of the different fuels are known.

The greater or less rapidity with which heat is disengaged from the combustibles, will be known by comparing the times of the experiments respectively.

In order to find the difference in the quantity or weight of combustible matter of different kinds, proper to produce the equally-elevated temperature, it is necessary to take of one of the combustibles, for example, wood, a sufficient quantity, suppose one cubic foot. This is to be set on fire in the stove, after it hath been filled with water, and the temperature noted. The thermometer determines the period at which the water boils; and, at this period, the fire must be extinguished, and all the fuel taken out which remains on the grate. And when the whole has been brought to its first temperature, the process must be repeated with the other combustibles; for example, turf or pit-coal.

and by the quantities of fuel consumed their economy is determined.

If, after the operation, the quantities of combustibles made use of be estimated at a medium price, it will be easy to show the cost of one compared with that of the other, and, consequently, what fuel is the least expensive.

We may also observe, that the external pipe *m*, may be made of wood; but if it be plate-iron or copper, it will be proper to cover it with a number of sheets of paper, forming a thickness sufficient to prevent the ready escape of heat.

The pipes, *k k* and *m m*, may be lengthened at pleasure, because a considerable portion of heat escapes through the aperture C.

The economical use of the present apparatus.

This apparatus may be used for different purposes; such as that of boiling water at a small expence. It is of great utility in domestic concerns. In order that its effect may be complete, the heated air ought to be deprived as much as possible of its caloric. The author, or perhaps the editors of the *Journal des Mines,* proceed to observe, that the cooled air being heavier than that of the atmosphere, causes the current in this kind of stove, and therefore they recommend that the descending tube should be made as long as local convenience will allow. It would not be needful to take notice of this oversight, if it were not accompanied with the practical deduction. The current is, in fact, produced by the rarefaction of that part of the air which ascends, and not by any increased density in the descending part, which, by the condition of the experiment, is, for the most part, in contact with hot water, and never colder than the surrounding atmosphere.

Error of the in-inventors.

III.

Letter from Mr. HUME, *of Long Acre, respecting the Carburetted Hydrogen Gas procured from Coals, by Dr.* CLAYTON, *early in the last Century.*

To Mr. NICHOLSON.

SIR,

AS an addition to the information already before the public, respecting the Hydrogen, or Carburetted Hydrogen Gas procured from Coals, it may not be improper to refer at once to an authority, beyond all others the most authentic and easy of access, I mean the Philosophical Transactions. In the 41st volume of that work, p. 59, there is a short paper on this subject, describing how the discovery originated, and some of the effects produced by this gas, or *spirit* of *Coals.* The paper appears to have been read before the Royal Society, in January, 1739, as, "a letter to the Hon. Robert Boyle, from the late Rev. John Clayton, D. D."

Dr. Clayton's Discovery of the inflammable Gas from Coals.

This letter is evidently a posthumous publication, and therefore may have been copied from that quoted by your correspondent Mr. Webster. However, lest there be any doubt, one being by *John*, the other by *James* Clayton, it is but fair to make both authorities known, in order that the merit of this discovery may no longer be disputed, nor claimed by any person living.

I am, Sir,
With much respect,
Your obedient Servant,
Long Acre, Feb. 10, 1807. JOS. HUME.

IV.

Curious Observations on the Wind, by ROGER ASHAM. *In a Letter from a Correspondent*

To Mr. NICHOLSON.

SIR,

Introductory Letter respecting a Quotation from Roger Ascham.

IN the English works of Roger Ascham, which were reprinted at London, in quarto, anno 1761, under the care of James Bennett, I find a number of curious particulars; one of which I am tempted to send, for the information of your readers. In his Toxophilus, or School of Shooting, which relates to Archery, the subject is handled in a manner truly scientific and orderly, and such as is eminently calculated to show by what care and attention our ancestors obtained their pre-eminence in that celebrated art. The passage I now send you constitutes part of a dissertation on the effects which the direction and force of the wind, and the state of the air, may have in preventing the archer from striking his mark. In our time, these observations will be taken as bearing a more general relation to the mass of atmospheric phenomena. But I will not detain you with longer preface. I copy from p. 168, but do not follow the ancient orthography.

I am, Sir,
Your obedient Servant,
R. B.

" The

Course of the wind.

" The wind is sometimes plain up and down*, which is commonly most certain, and requires least knowledge, wherein a mean shooter, with mean gear, if he can shoot home, may make best shift. A side wind tries an archer and good gear very much. Sometimes it blows aloft, sometimes hard by the ground, sometimes it bloweth by blasts, and sometimes it continues all in one; sometimes full side wind, sometimes quarter with him and more, and likewise against him, as a man with casting up light grass, or else, if he take good heed, he shall sensibly learn by experience. To see the wind with a man's eye, it is impossible, the nature of it is so fine and subtle; yet this experience had I once myself, and that was in the great snow that fell four years ago (1540). I rode in the highway betwixt Topcliffe upon Swale and Borowbridge, the way being somewhat trodden before by wayfaring men: the fields on both sides were plain, and lay almost yard deep with snow: the night before had been a little frosty, so that the snow was hard and crusted above. That morning the sun shone bright and clear, the wind was whistling aloft, and sharp, according to the time of the year: the snow in the highway lay loose, and trodden with horses' feet, so as the wind blew, it took the loose snow with it, and made it so slide upon the snow in the fields, which was hard and crusted by reason of the frost over night, that thereby I might see very well the whole nature of the wind as it blew that day, and I had a great delight and pleasure to mark it, which makes me now far better to remember it. Sometimes the wind would be not past two yards broad, and so it would carry the snow as far as I could see. Another time, the snow would blow over half the field at once; sometimes the snow would tumble softly, by and by it would fly wonderfully fast. And I also perceived that the wind goes by streams, and not together; for I could see one stream within a score of me, then the space of two score no snow would stir. But after so much quantity of ground, another stream of snow at the same time should be carried likewise, but not equally; for the one would stand still when the other flew apace, and so continue, sometimes swifter, sometimes slower, sometimes broader, sometimes narrower, as far as I could see. Now it flew straight, but sometimes crooked this way,

— observed on the surface of the snow.

— varying in the extent and velocity of its several streams way, and sometimes it ran round about in a compass. And sometimes the snow would be lifted clean from the ground up to the air, and by and by it would be all clapt to the ground, as though there had been no wind at all; straightway it would rise and fly again. And that, which was the most marvellous of all, at one time two drifts of snow flew, the one out of the west into the east, the other out of the north into the east. And I saw two winds by reason of the snow, the one cross over the other as it had been two highways; and again, I heard the wind blow in the air, when nothing was stirred at the ground. And when all was still where I rode, not very far from me, the snow should be lifted wonderfully. This experience made me more marvel at the nature of the wind, that it made me cunning in the knowledge of the wind; but yet thereby I learned perfectly that it is no marvel at all, though men in wind lose their length in shooting, seeing so many ways the wind is so variable in blowing.

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* From the context it appears that, by *plain up and down*, the Author means *directly to or from the mark*.

—and also in direction, &c.

Two currents of air at the same time

" But seeing that the master of a ship, be he never so cunning, by the uncertainty of the wind, loses many times both life and goods, surely it is no wonder, though a right good archer, by the selfsame wind, so variable in its own nature, so insensible to our nature, loses many a shot and game.

V.

Observations on the Marine Barometer, made during the Examination of the Coasts of New Holland, and New South Wales, in the years 1801, 1802, *and* 1803. *By* MATHEW FLINDERS, *Esq. Commander of his Majesty's Ship Investigator, in a Letter to the Right Honourable Sir* JOSEPH BANKS, *Bart. K. B. P. R. S., &c. &c. From Philosiphical Transactions for* 1806.

[Concluded from Page 118.]

THE greatest range of the mercury observed upon the last coast, was from 29, 60 to 30, 36 at Port Jackson; and within the tropic from 29, 88 to 30, 30; whilst upon the south coast, the range was from 29, 42 to 30, 51, in the western part, where the latitude very little exceeds that of Port Jackson. It is to be observed, however, that these extremes are taken for very short intervals of time.

Observations and inferences to ascertain the correspondent changes of wind and weather, to be expected after change in the marine barometer.

My

Observations and inferences to ascertain the correspondent changes of wind and weather, to be expected after change in the marine barometer.

My observations upon the north coast of Australia are but little satisfactory, both because the changes in the barometer were very small in so low a latitude, and that very little more than the shores of the gulph of Carpentaria could be examined on account of the decayed state of the Investigator, which obliged me to return with all practicable expedition to Port Jackson. An abridged statement, however, of the general height of the mercury under the five following circumstances, will afford some light upon the subject, and perhaps not be uninteresting. 1st. On the east side of the gulph, and at the head, with the south-east monsoon, or trade wind. 2d. At the head of the gulph with the north-west monsoon. 3d. On the west side during the north-west monsoon. 4th. At Cape Arnhem under the same circumstance; and 5th. In the passage from Cape Arnhem, at a distance from the coast, to Timor, with variable winds.

In a memoir written by Alexander Dalrymple, Esq. F. R. S. respecting the Investigator's voyage, there is this general remark :—" Within the tropics, the monsoon blowing on " the coast produces rainy weather, and when blowing from " over the land, it produces land and sea breezes." This I found verified on the east side of the gulph of Carpentaria, between November 3 and 16, which time was employed in its examination ; for though we had found the south-east trade to blow constantly on the east side of Cape York just before, and doubtless it did so then, yet in the gulph we had a tolerably regular sea breeze, which set in from the westward at eleven or twelve o'clock, and continued till seven, eight, or nine in the evening Towards the head of the gulph, the trade wind, which blew at night and in the morning, came more from the NE, and the sea breezes more from north and NW, but without producing any regular alteration in the height of the mercury, whose average standard was 29,95 ; it never fell below 29,90 or rose above 30,04. At the head, the height of the mercury remained nearly the same, until the north-west monsoon began to blow steadily, about the 10th of December, two or three days excepted, when the day winds were from the south-eastward, and the mercury then stood between 29,80 and 29,85. At these times, however, there was usually some thunder and lightning about, signs of the approaching rainy monsoon, which may perhaps account for the descent of the mercury independently of the direction of the wind.

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2d. On the confirmation of the north-west monsoon, there was a change in the barometer at the head of the gulph, the common standard of the mercury being at 29,88; but during the times of heavy rain, with thunder, lightning, and squalls of wind, when amongst the islands of Cape Vanderlin, the mean height was 29,79. The north-west monsoon, after coming over Arnhem's Land, blows along the shore for a considerable part of the space between the Cape Maria and Cape Van Diemen, of Tasman; and during the examination of the parts so circumstanced, we sometimes had tolerably fine weather, and the mercury above 29,90; but the wind was then usually more from the north than when the mercury stood lower. As we approached Cape Maria, and the bight between it and the south side of Groote Eyland, the mercury stood gradually lower; and in the bight, where the north-west monsoon came directly from off the shore, although we had sea and land breezes with fine weather, according to Mr. Dalrymple's general position, yet the mercury was uncommonly low, its range being from 29,63 to 29,81: the average 29,74, below what it had stood in the very bad weather near Cape Vanderlin. These winds and weather, and the low state of the mercury, continued until we got without side of Groote Eyland.

3d. On the east side of Groote Eyland, and the west side of the gulph, northward from that island, we sometimes had sea and land breezes with fine weather; we had also two moderate gales of wind from the eastward, of from two to four days continuance each, with one of which there were heavy squalls of wind and rain; sometimes also, the winds were tolerably steady between north and west, with fine weather. During all these variations, the mercury never differed much from its average standard 29,90; and it seemed as if the increase of density in the air, from the wind blowing upon the coast, was equal to its diminution of quantity from the fall of rain and strength of the wind; and, on the other side, that the wind from over that corner of Arnhem's Land permitted the mercury to descend, as much as the fine weather would otherwise have occasioned it to rise.

Upon the north side of Groote Eyland, the mercury stood higher

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higher than usual for five days, and during this time the wind blew with more regularity from NW, the only exception being for a few hours in the afternoons, when it commonly sprung up from the NE in the manner of a sea breeze: the weather remained fine during these five days, and the height of the mercury averaged 29,94.

4th. In the neighbourhood of Cape Arnhem, the mercury usually stood about 29,90, whether the wind was from NW, NE, or east, if the weather was fine; but if by chance the wind shifted to the south side of west, off the land, it descended to 29,80 though the weather remained the same: and this was its standard during those times when strong gusts came from the NW accompanied with heavy rain, thunder, and lightning.

In this example, the wind from SW occasioned the mercury to stand lower than that from NW in the same weather; which is contrary to what was observed upon the south and east coasts; particularly on the former, where the south-west wind elevated the mercury up to, and sometimes above 30,25.

5th. On March 6, 1803, we made sail off from the north coast, towards Timor, the north-west monsoon having ceased to blow at Cape Arnhem, and the eastwardly winds appearing to have set in; but we soon outran them, and had the wind so variable and light afterwards, that it took us twenty-three days to reach Coepang Bay, a distance of no more than 12° of longitude. The only two remarks I made upon the barometer during this passage were, that the common height of the mercury was 29,95 at those times that the wind remained steady for some hours, from whatever quarter it came, and about 29,85 when it was most unsettled; and that it stood higher, upon the average, after we had passed Cape Van Diemen, when the south-west winds, which blew oftenest, came from the sea, than it did before.

The medium height of the mercury, deducting the time between Cape Maria and Groote Eyland in the 2d example, I should take at 29,92, which, when the quantity of rainy squally weather, with thunder and lightning, is considered, is very high: the whole range of the mercury upon the north coast was four-tenths of an inch.

The principal differences in the effect of winds upon this coast, from what they produced upon the south and east coasts, are, that a north-east wind raised the mercury as high, if not higher, than one from the SE; and that a north-west wind, where it came from off the sea and was moderate, was equal to either of them, and kept it up higher than the south-west wind did.

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In order to have ascertained the full effects of sea and land winds upon the barometer, it was desirable to have learned, whether the south-east winds, which occasioned the mercury to rise highest upon the south and east coasts, would have left it at the medium standard, or made it descend upon the north-west and west coasts of Australia; but, unfortunately, the state of the ship did not permit me to determine this; for at the distance we kept from these coasts, in making the best of our way to Port Jackson, the accumulation of air over the shore, arising from a sea wind, or the contrary from a land wind, can scarcely be supposed to have much, if any effect. The principal winds we experienced between Timor and Cape Leuwen, in the months of April and May, were from SE and SW. The south-east wind prevailed as far as the latitude 25°, and the mercury stood at first with it at 29,95; but as we advanced southward, it rose gradually to 30,25, nearly in the same way as it had before descended on the east side of Australia, when we steered northward in the month of October. This wind was succeeded by an unsteady northwardly wind, which brought the mercury down to 29,90; but on its veering by the west to SW it rose fast, and fixed itself about 30,32: we were then drawing near Cape Leuwen.

As far as this example can be admitted in proof, it appears, that a wind from the SW has an equal, if not a superior power to one at SE in raising the mercury upon the west coast, which was not the case upon the south, and still much less upon the east and north coasts, where the south-west wind caused it to fall. Winds from the northward caused the mercury to descend, as I belive they always will in the southern hemisphere, if not obstructed by the land; but upon the north coast, we have seen the mercury stand higher with it than almost any other.

Upon a summary of the effects of the same winds upon the

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different coasts of Australia, as deduced from the above examples, the following queries seem to present themselves:

Why do the winds from north and NW, which cause the mercury to descend and stand lower than any other upon the south and east coasts, and also in the open sea, and in the south-west bight of the gulph of Carpentaria, make it rise upon the outer part of the north coast, with the same, or even worse weather?

Why should the north-east wind, which occasions a fall in the barometer upon the south coast, considerably below the mean standard, be attended with a rise above the mean upon the east and north coasts?

The south-east wind, upon the south and east coasts, caused the mercury to rise higher than any other; why should it not have the same effect upon the north coast, and upon the west?

How is it that the south-west wind should make the quicksilver rise and stand high upon the south and west coasts,—should cause it to fall much below the mean standard upon the east coast,—and upon the north, make it descend lower than any other, with the same weather?

The answer, I think, can only be one; and it seems to be sufficiently obvious.

The cause of the sensibility of the mercury to winds blowing from the sea and from off the land, may perhaps admit of more than one explanation; but the following seems to me to be direct, and tolerable satisfactory. The lower air, when brought in by a wind from the sea, meets with resistance in passing over the land; and to overcome this resistance, it is obliged to rise, and it will make itself room by forcing the superincumbent air upwards. The first body of air, that thus comes in from the sea, being itself obstructed in its velocity, will obstruct the second, which will therefore rise over the first [illegible]ke manner to overcome the obstruction; and as the course of the second body of air will be more direct towards the top of the highest part of the land it has to surmount, than the first was, so the first part of the second body will arrive at the top, before the latter part of the first body has reached it; and this latter part will be able to pass over the top, being kept down by the second body and the successive stream of air, whose velocity is superior to it. In this manner, an eddy, or body of compressed, and comparatively inactive air will be formed, which, at first, will occupy all the space below a line drawn from the shore to the top of the highest land: but, almost immediately, the succeeding bodies of air, at a distance from the shore, will feel the effect of the obstruction; and being impelled by those that follow them, will begin to rise, taking their course for the top of the highest land, before they come to the shore; by which means, the stratum of lower air will be deeper between the top of the land and the shore, and to some distance out from it, than it is either upon the mountains or in the open sea. If this is admitted to be a necessary consequence of a wind blowing upon the shore from the sea, it follows, that the mercury ought to stand something higher when such a wind blows, whether it is from the south or any other quarter, than it will with the same wind where it meets no such obstruction; and the more direct it blows upon the coast, and the higher the land is, (all other circumstances being equal,) the higher ought the mercury to rise. On the other hand, when the wind comes from off the hills, this dead and dense air will be displaced, even from its hollows under the highest land; both on account of its own expansion, and because its particles will be attracted by those of the air immediately above, which are taking their unobstructed course out to sea; and thus the air over the coast will resume its natural state with a land wind.

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In order to appreciate duly the effect of sea and land winds upon the barometer, in the preceding examples, it is necessary to be recollected, that in the southern hemisphere, a wind from the south has a natural tendency to raise the mercury in the open sea, and one from the north to depress it; probably, from the superior density of the air brought in by the former; therefore, if the mercury rises quicker and higher with a south wind upon the south coast, then it does with a north wind upon the north, it is not to be at once concluded, that the effect of the wind as coming from the sea, is less upon the north coast; for it has, in the first place, to counteract the tendency of the mercury to fall with a north wind; and, in some cases, its effects as a sea wind may be as considerable, relatively to the latitude, where there shall be no rise in the barometer, as upon the south coast it might where a considerable one took place. The same thing may be said of the winds from the east and from the west; for where the vicinity of land is out of the question, the former generally causes an ascent (from what principle, I leave others to determine,) and the latter a descent in the barometer, and I

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believe this extends to both hemispheres, and all climates. The wind from SE then, which combines something more than half the power, both of the south and of the east wind, will raise the mercury higher than any other on the south side of the equator, and the wind from NW permit it to fall lower, independantly of their effects as sea and land winds; and this allowance requires to be first made upon them: the south-west and north-east quarters should be equal where there is no land in question, and of a medium strength between the power of the south-east, and the deficiency of the north-west wind.

I leave it wholly undetermined, whether the effects of sea and land winds upon the barometer, as above described, extend beyond the shores of the country where these observations were made, and to about one hundred leagues of distance from them; but it seems not improbable, that they may be found to take place near the shores of all countries similarly circumstanced; that is, upon those which are wholly, or for the most part, surrounding by the sea, and situated within the fortieth degree of latitude. In colder climates, where snow lies upon the ground during a part of the year, the wind from off the land may perhaps be so cold, and the air so much condensed, as to produce a contrary effect; but this, and the prosecution of the subject to other important consequences, I leave to the philosopher; my aim being only to supply my small contribution of raw materials to the hands of the manufacturer, happy if he can make them subservient to the promotion of meteorological science.

I will conclude with stating a few general remarks upon the barometer, such as may be useful to seamen.

It is not so much the absolute, as the relative height of the mercury, and its state of rising and falling, that is to be attended to in forming a judgment of the weather that will succeed; for it appears to stand at different heights, with the same wind and weather, in different latitudes.

In the open sea, it seems to be the changes in the weather, and in the strength of the wind, that principally affect the barometer; but near the shore, a change in the direction of the wind seems to affect it full as much, or more, than either of those causes taken singly.

It is upon the south and east coasts of any country in the southern, or the north and east coasts in the northern hemisphere,

sphere, where the effect of sea and land winds upon the barometer is likely to be the most conspicuous.

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In the open sea, the mercury seems to stand higher in a steady breeze of several days continuance, from whatever quarter it comes, provided it does not blow hard, than when the wind is variable from one part of the compass to another; and perhaps it is on this account, as well as from the direction of the wind, that the mercury stands higher within the tropics, than, upon the average, it appears to do in those parallels where the winds are variable and occasionally blow with violence.

The barometer seems capable of affording so much assistance to the commander of a ship, in warning him of the approach and termination of bad weather, and of changes in the direction of the wind, even in the present state of meteorological knowledge, that no officer in a long voyage should be without one. Some experience is required to understand its language, and it will be always necessary to compare the state of the mercury with the appearance of the weather, before its prognostications will commonly be understood; for a rise may foretel an abatement of wind,—a change in its direction,—or the return of fine weather; or, if the wind is light and variable, it may foretel its increase to a steady breeze, especially if there is any easting in it; and a fall may prognosticate a strong breeze or gale, a change of wind, the approach of rain, or the dying away of a steady breeze. Most seamen are tolerably good judges of the appearance of the weather; and this judgment, assisted by observation upon the quick or slower rising or falling of the mercury, and upon its relative height, will in most cases enable them to fix upon which of these changes are about to take place, and to what extent, where there is only one; but a combination of changes will be found more difficult, especially where the effect of one upon the barometer is counteracted by the other: as for instance, the alteration of a moderate breeze from the westward with dull, or rainy weather, to a fresh breeze from the eastward with fine weather, may not cause any alteration in the height of the mercury; though I think there would usually be some rise in this case. Many combinations of changes might be mentioned, in which no alteration in the barometer would be expected, as a little consideration, or experience in the use of this instrument, will make sufficiently evident; the barometer alone, therefore, is not sufficient; but in assisting the judgment of the seamen, is capable of rendering very important services to navigation.

VI.

Analysis of the Substance known by the name of Turquois. By M. BOUILLON LAGRANGE.

MANY Mineralogists have placed the turquois among calcareous bodies and the genus called opake; and others, on account of their blue or green colour, have classed them among the ores of copper.

Chaptal's account of the Turquois stone

" The turquoises, (says M. Chaptal) are merely bones coloured by the oxides of copper. The colour of the turquois often passes to green, which depends on the state of the metallic oxide; the turquois of Lower Languedoc emits a fetid smell by the action of fire, and is decomposed by acids; the turquois of Persia emits no odour, and is not attacked by acids. Sage suspects that the osseous part is azatized in those left."

Places where they are found

Many turquoises are found in Persia, but none in Turkey, as its name seems to imply. They are obtained from two mines: the one, called the Old Rock *, at three days journey to the north-west of Meched, near Nichaburgh; the other, at five days journey, is called the New Rock. The Turquoises of this last place, are of a bad blue, inclining to white, and are therefore cheap. But since the close of the last century, the King of Persia has prohibited the Old Rock to be explored, except for himself; because the workmen of the country working only in wire (*en fil*), and not being acquainted with the art of enamelling on gold, they made use of it for the mounting of sabres, poignards, and other tools, turquoises of this mine, instead of enamel, by cutting and setting them in different forms.

I shall add some other details, extracted from different works;

* Annales de Chymie, LIX, 180.
La Harpe's Abrégé des Voyages, VI, 507.

works; in which I am indebted to the kindness of the celebrated mineralogist M. Haüy.

Turkis (turquois) Reuss, page 511, part 2, vol. 3.

The Turquois has always been considered as the tooth of an unknown animal, of which the sky-blue colour depends on oxide of copper; or, according to others, on oxide of iron; which has caused it to be ranked in the calcareous order, and sometimes in that of copper, as animal petrefaction (odontalite.)

Various accounts of the turquois, by different authors.

Lommer, in the *Abhandlungen einer Privat-Gesellschaft in Bæhmen*, 2 vol. page 112, 118, thinks the turquois is a produce of art. He asserts, that a tooth found in the neighbourhood of Lissa, in Polomie, being exposed to a strong heat in the muffle of an assayer's furnace, became converted into a turquois; and he recommends the heat to be very gradually raised, for fear the tooth should fly in pieces.

Bruckman gives a complete history of all that has been written from Pluds to Lommer, on the turquois. He mentions mount Caucasus as a place of origin, at the distance of four days journey from the Caspian Sea; where, according to Chaidin, this stone is dug up. It is likewise in Persia, Egypt, Arabia, and in the province of Samaveande.

Supposition that the turquois is not an animal substance.

Dambsy brought it from Peru; some of them contained native silver.

The occidental turquois is found in France, at Simore, in Lower Languedoc, in Bohemia, in Siberia, and in Hungary.

Demetrius Agaphi, who visited the place where the turquois is found, near Chorasen, in the neighbourhood of the town of Pishepure, relates, in the fifth volume *der Nordischen Beiträge*, 1793, page 261, that the turquois is found in a stone as its matrix, in masses and small points; and that it might be considered as a peculiar mineral, which has the same situations as opal, the chrysophane, and the resiniform quartz.

Mr. Bruckman, in *Crell's Chemical Journal*, 1799, vol. 2, page 188 to 199, thinks, from the nature of its position at Chorasan, and after the analysis at Lametz, that the turquois is not a petrefaction of parts of animals, but a particular mineral.

Lonsitz obtained from it, by analysis, much clay, a little copper, and iron; but neither lime, nor phosphoric acid *.

According

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* I do not know whether the substance analyzed by M. Lowitz,

The oriental turquois supposed not to be a petrefaction.

According to Meder, the oriental turquois is found in a primitive argillaceous schistus, of a grey bluish, or black greyish colour, which excludes all supposition of petrefaction. Graphic schistus, and quartz, are found in the same place. In the argillaceous schistus, the turquois is found disseminated; and it is the same with quartz, and the graphic schistus.

To remove every idea that the turquois cannot be considered as malachite, or green copper (Kupfergrün), Meder has given the following character:

Its colour is apple green greyish; when it begins to soften, it is decomposed, and assumes a mountain-green colour; when completely decomposed, it is of a yellowish white, green, and near straw colour.

It is commonly found disseminated in small superficial parts, and seldom in masses; its interior structure is dull, or scarcely sublucid; its fracture compact, and the fragments irregular with sharp edges, opake when it is decomposed, and more or less transparent at the edges. Its hardness varies according to the degrees of its decomposition, it is easily broken, and its specific gravity, according to Curwan, is between 2,500, and 2,908.

The turquoises are not all of equal hardness: this must be attributed to the differences of the boney substances which constitute their base. The degree of petrefaction must also influence this property.

The turquois, in the solid form, is sometimes mixed with the brown earthy oxides of copper.

M. Meder infers, from all these characters, that the turquois ought to be placed between the opal and the chrysopaze, with which it appears to agree by the varieties of green.

Cuivier considers the turquois, as fossil teeth, coloured by copper.

Lastly, the celebrated Cuivier, in the *Journal de Physique*, page 263, vol. 52, thinks that the turquoises, namely, those which are found near Simore, in Languedoc, and near Trévoux, are the superferous teeth of an animal, resembling that which has been found near the Ohio, or the mammoth of the English and Americans; the carnivorous elephant.

Mr. Reaumur alone has given some detail respecting the mines of turquoises, and the nature of the substances there found.

His

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ought to be considered as a turquois, or as a particular mineral; but I am more disposed to think it was not as in all the turquois. I have examined, and found lime and phosphoric acid. D. L.

His memoir is printed among those of the Royal Academy of Sciences, for the year 1715, which may be consulted for every thing which bears relation to the situation of the mines, and the extraction of the turquoises.

Reference to the mines of turquoises, &c.

With regard to the experiments made by the author, in order to discolour these substances, though not very conclusive, it appears to me, nevertheless useful, to bring together these facts along with the means which I have employed to ascertain the nature of the stone. I shall first present a few fragments of this part of Reaumur's memoir.

Reaumur's observations on the colouring matter.

The colouring matter, says the author, which fills the cellules of the turquois, and which afterwards tinges the whole stone, is, no doubt, a particular substance; but is it a simple mineral matter, like cobalt, or the material from which azure and sapphor are made, from which the finest blue of porcelain, and pottery is served; or, is it a metallic matter? I have not been able to satisfy myself in this respect.

It was taken up by vinegar.

I at first suspected that our turquoises might probably derive their colour from copper. This metal is capable of affording a blue, and a green . . . . . . But I have found that the turquoises may be extracted like that of coral: of all the solvents, which I have used, distilled vinegar succeeded the best. If a thin piece of turquois be steeped in this vinegar, its angles, after an hour or two, become white; and in two or three days, the whole of the upper surface of the stone, and even its internal parts, assume the same colour.

Vinegar, while it extracts the colour, likewise dissolves the stone; it is always covered with a kind of white cream, composed of parts which have been detached. Juice of lemons likewise dissolves this kind of stone, but it only weakens the colour; and that which is found under the kind of cream, we have described, is blue, when the stone has been put into this liquid.

Aquafortis, &c. applied to these stones.

As to aquafortis, and aquaregia, they are not proper to extract the colour from our turquoises; they very speedily dissolve the whole substance of the stone, but they afford the means of distinguishing the Persian turquoises from those of France. Aquafortis does not act upon those of Persia; whence it follows, that these two kinds of stone, though similar in appearance, are nevertheless of a very different nature; it would be wrong, however, to draw a consequence to the disadvantage tange of our stones, by concluding that they have less tenacity.

Aquaregia likewise acts differently upon these two kinds of stone. It totally dissolves ours, and it reduces those of Persia into a kind of paste, more whitish than the turquois was, but which is not, nevertheless, deprived of all its blue colour.

Turquoises lose their colour, and their value by time.

In general, this kind of stone has a singular defect; namely, that, without the assistance of any other agent than that of time, their colour changes: insensibly their blue assumes a shade of green, they become greenish, and at last green; whereas the colour of other precious stones is unchangeable. When the turquoises have become green, they are no longer of any value; the convention of society has placed them in no estimation whatever with that colour.

Chemical Examination.

Characters of the turquois.

Physical characters. Specific gravity, 3.127.—Colour, light green and blue; surface, smooth or polished; hardness, such as slightly to scratch glass; difficult to be pounded; powder, greenish grey; fracture, polished.

Habitude with the blow-pipe.

Chemical characters. Before the blow-pipe, it loses its colour, and becomes of a greyish white, but does not melt.

Heated in a crucible of platina, it acquires the same colour, but becomes friable, and is easily reduced to powder. In this experiment it loses 6 per cent. of its weight.

Soluble in mineral acids.

The nitric and muriatic acids totally dissolve the turquois. The solution, in the latter acid, is yellow; and that in the nitric, is colourless.

Nitric solution.

The nitric solution presented the following phænomenon:—1, with lime-water, a white flaky precipitate—2, by ammonia in excess, a precipitate of the same colour, but more abundant: the supernatant fluid did not acquire any bluish tinge—3, carbonate of ammonia likewise gave a precipitate—4. with the oxalate of ammonia, the precipitate was very light and very divided—5. precipitate of pot-ash gave a deep blue precipitate.

These preliminary experiments already afford an approximation to a knowledge of the constituent parts of the turquois; they are not sufficient to lead to a regular classification. I therefore chose out of a certain quantity of turquoises, those which were the most coloured and the most hard, and I submitted them to the following experiments:

Analysis solution in nitric acid carbonic acid escaped.

A.—100 parts of turquoises, reduced to powder, were introduced into a small retort: and 300 parts of nitric acid, at 36 degrees, were poured in. After some time, a slight effervescence appeared, which lasted till the solution was complete. The gas being collected in the pneumatic apparatus with mercury, presented all the characters of carbonic acid gas.

B.—This nitric solution is white, and of the consistence of syrrup. It was then evaporated to dryness, and the remaining matter made red-hot in a crucible of platina.

The dissolved matter was a phosphate.

C.—The calcination had scarcely changed its colour.

This substance was again dissolved in water, acidulated with nitric acid, with the intention of separating the iron, which might exist in the state of oxide. But the whole was entirely dissolved, which evidently proves that the iron was neither in the state of red oxide nor in that of nitric, but in that of phosphate.

Small portion of alumine.

D.—Ammonia in excess was poured on the liquor C, which gave a white precipitate of considerable bulk. This precipitate, after washing and drying, was treated with concentrated liquid pot-ash, which dissolved a certain quantity. The liquor of the non-dissolved portion was afterwards separated from the liquor, and muriate of ammonia added, which separated a white substance, possessing all the properties of alumine. This substance, after the calcination, weighed one part and a half.

E.—The portion dissolved by the pot-ash was also calcined, and its weight proved to be 82 parts.

Lime.

F.—Being desirous of ascertaining whether the liquor, from experiment D, did not contain lime in solution, carbonate of ammonia was poured on the fluid, and a precipitate was obtained, which, being slightly dried and heated, was found to be carbonate of lime. Its weight was 8 parts.

G.—The supernatant liquor was afterwards evaporated, but it afforded no precipitate; whence it may be concluded, that it contained no magnesia.

Iron.

H.—Being persuaded beforehand that the precipitate E contained phosphates, it was treated with the sulphuric acid. The matter was afterwards washed, and the waters being put together, precipitate of pot-ash was poured on, which formed a precipitate of a fine deep blue, of which the weight, after calcination, was found to be one part and a half. It was red oxide of iron. Care must be taken to heat the liquor, in order to separate the precipitate entirely.

The supernatant liquor held in solution the acid phosphate of lime, which was shewn by the phosphorus it afforded, when treated with charcoal.

A trace of manganese,

I.—This oxide of iron was heated again with a little pure pot-ash. When the whole was in fusion, the matter assumed a deep green colour, and when the cold mass was afterwards dissolved in water, it gave the same colour to the fluid. Upon adding a small quantity of muriatic acid, it became of a fine rose colour. This experiment was repeated on a number of turquoises, and the phænomenon always took place; which evidently shows the presence of a very small quantity of magnesia.

—and manganese.

K.—Being desirous of ascertaining whether the turquois contained phosphate of magnesia, as the experiments of Fourcroy[*] and Vauquelin upon bones, lead to suspect, I treated this substance according to the method indicated by those chemists, in the 47th volume of the *Annales de Chymie*. It was found that 100 parts of the turquois contained two parts of the phosphate of magnesia.

Component parts.

From the preceding experiments, it follows that 100 parts of turquois contain

Phosphate of lime	80
Instead of 82, found in experiment E.	
Deducting the quantity of phosphate of magnesia, before mentioned	0
Carbonate of lime	8
Phosphate of iron	2
of magnesia	2
of manganese, minute quantity	0
Alumine	1
Water and loss	6
	100

Whether all the turquoises be of the same nature as those here examined

Though I obtained similar products in the examination of several turquoises, it cannot yet be decided whether they be indentical. The turquoises used in my experiment are perfectly similar to those in the Cabinet of Museum of Natural History; and M. Hauy, whom I consulted, could not affirm whether they were truly from Prussia. M. Guyton thinks that there is a difference between the turquoises of Persia and the Occidental.

This philosopher has announced, for several years, in his course of mineralogy, at the Polytechnic School, that the former contained silex. It is possible that turquoises may contain this earth accidentally; but I have not found it in any of those which I examined. This difference ought not, I think, to suspend the classification of this substance by mineralogists. M. Guyton himself has already placed it among fossil bones. This celebrated chemist has likewise made some comparative experiments. He has found that fossil bones assume, in the fire, a colour similar to that of turquoises; that, when digested in water containing pot-ash, they turn blue; and that this blue varies in its shade, by passing from greenish blue to deep blue; and, lastly, that bones, exposed to the air, become white.

Messrs. Fourcroy and Vauquelin have likewise observed, that bones, strongly calcined, often assume a bluish tinge: this colour appeared to them to be owing to the presence of a small quantity of phosphate of iron.

Observation and experiments on iron, as a colouring matter.

There cannot, therefore, any longer exist a doubt respecting the matter which colours the turquoises. If it were necessary to add any thing more to the facts announced, I should observe, that having put the same turquoises which I analyzed, into the hands of Mr. Vauquelin, he did not find a particle of copper in them; and, lastly, I have ascertained that, by pouring into a solution of muriate of lime, phosphate of soda and some drops of muriate of iron at the maximum, the phosphate of lime and of iron is obtained, of which the colour is a greenish blue. We may, likewise, by decomposing the phosphate of soda by muriate of iron at the maximum, obtain a phosphate of iron, which is not white, as some chemists have asserted, but of a green bluish colour.

These reflections, without doubt, are not very important; but I present them as tending to show the possibility of imitating the colour of the turquois, and at the same time to show that iron can, in various circumstances, afford colours similar to those of copper.

VII.

VII.

Account of MR. CURWEN'S METHOD *of* FEEDING COWS, *during the Winter Season, with a View to provide poor Persons and Children with Milk at that time.*

SIR,

Preface.

EVERY attempt to ameliorate the condition of the labouring classes of the community, is an object not unworthy of public attention; and has, on all occasions, been zealously patronised by the Society of Arts. Under this impression I hope for the indulgence of the Society in calling their attention to an experiment, which I flatter myself will, in its consequence, prove not only highly beneficial to the lower orders of society, but tend likewise to the advancement of agriculture.

Great benefits to be expected from a plentiful supply of milk

There is not any thing, I humbly conceive, which would conduce more essentially to the comfort and health of the labouring community and their families, than being able to procure, especially in winter, a constant and plentiful supply of good and nutritious milk. Under this conviction, much pains have been taken to induce the landed proprietors to assign ground to their cottagers, to enable them to keep a milch cow. The plan is humane and highly meritorious; but unfortunately its beneficial influence can reach but a few, Could farmers in general be induced from humanity, or bound by their landlords to furnish milk to those, at least whom they employ, it would be more generally serviceable. Even those who have the comfort of a milch cow, would find this a better and cheaper supply, as they can seldom furnish themselves with milk through the winter. The farmer can keep his milch cows cheaper and better; for, besides having green food, his refuse corn and chaff, of little value, are highly serviceable in feeding milch cows.

General notion that dairies are not profitable in winter.

My object is to combat the prevailing opinion, that dairies in summer are more profitable than in winter. I confidently hope to establish a contrary fact. The experiment I am about to submit to the Society, is to prove, that by adopting a different method of feeding milch cows in winter, to what is in general practice, a very ample profit is to be made, equal, i not superior to that made in any other season.

I believe the principle will hold good equally in all situations: my experience is confined to the neighbourhood of a large and populous town.

The price of milk is one-fifth higher in winter than in summer. By wine measure the price is 2*d.* per quart new milk, 1*d.* skimmed.

Local situation of the author.

My local situation afforded me ample means of knowing how greatly the lower orders suffered from being unable to procure a supply of milk; and I am fully persuaded of the correctness of the statement, that the labouring poor lose a number of their children from the want of a food so pre-eminently adapted to their support.

Experiments of a different method of feeding.

Stimulated by the desire of making my farming persuits contribute to the comfort of the public, and of those by whose means my farm has been made productive, I determined to try the experiment of feeding milch cows after a method very different to what was in general practice. I hoped to be enabled thereby to furnish a plentiful supply of good and palatable milk, with a prospect of its affording a fair return of profit, so as to induce others to follow my example.

The supply of milk, during the greatest part of the year, in all the places in which I have any local knowledge, is scanty and precarious, and rather a matter of favour than of open traffic.

Provision.

Consonant with the views I entertained of feeding milch cows, I made a provision of cabbages, common and Swedish turnips, kholrabi, and cole seed. I made use also of chaff, boiled and mixed with refuse grain and oil cake. I used straw instead of hay for their fodder at night.

The greatest difficulty, which I have had to contend with, has been to prevent any decayed leaves being given. The ball only of the turnip was used. When these precautions were attended to, the milk and butter have been excellent.

Having had no previous knowledge of the management of a dairy, my first experiment was not conducted with that frugality requisite to produce much profit.

The first experiment.

I sold the first season, between October 1804, and the 10th of May 1805, upwards of 20,000 quarts of new milk. Though my return was not great, I felt a thorough conviction that it proceeded from errors in the conduct of the undertaking; and that, under more judicious management, it would not fail of making an ample return, which the subsequent experiment will prove. In the mean time I had the satisfaction of knowing that it had contributed essentially to the comfort of numbers.

In Oct. 1805, my dairy recommenced with a stock of 30 milch cows; a large proportion of these were heifers; and in general the stock was not well selected for giving milk; for they were purchased with a view of their being again sold as soon as the green crop should be exhausted. If the plan be found to answer under such unfavorable circumstances, what may not more experienced farmers expect?

By the end of this present month, I shall have sold upwards of 40,000 quarts of milk.

The quantity of food, and its cost, are as follow. The produce of milk from each cow upon 200 days, the period of the experiment, is calculated at no more than 6 wine quarts in the 24 hours: this is to allow for the risk and failure in milk of some of the heifers. A good stock, I have no doubt, would exceed 8 quarts in the two meals, which would add 100*l.* to the profit.

Daily cost of feeding one milch cow:

Accounts, statements of the feed and produce.

	£.	s.	d.
Two stone of green food (supposing 30 tons of green crop on an acre, at ¼*d.* per stone would pay 5*l.* per acre) at ¼*d.* per stone of 14*lb.* - -	0	0	0½
Two stone of chaff boiled, at 1*d.* per stone -	0	0	2
Two *lbs.* of oil cake at 1*d.* per *lb.* costing from 8*l.* to 9*l.* per ton - - -	0	0	2
Eight *lbs.* of straw at 2*d.* per stone - -	0	0	1
	0	0	5½

The chaff, beyond the expense of boiling, may be considered as entirely profit to the farmer; 2*d.* per stone for straw, likewise leaves a great profit. Turnips also pay the farmer very well at ¼*d.* per stone.

Expense of feeding one milch cow for 200 days, the period upon which the experiment is made:

	£.	s.	d.
200 days keep of one milch cow, at the rate of 5½*d.* per day	4	11	8
Attendance	2	0	0
Supposed loss on re-sale	2	0	0
	£.8	11	8

Return made of one milch Cow in 200 days milking:

	£.	s.	d.
6 quarts per day, at 2*d.* per quart, for 200 days	10	0	0
Calf	2	0	0
Profit on 20 carts of manure, 1*s.* 6*d.* each	1	10	0
	13	10	0
Clear gain upon each milch cow	£.4	18	4

Observation on the considerable profit obtained.

This gives a profit upon the whole stock of £.147 10*s.* The profit of another month may be added before a supply of milk can be had from grass, which will make the balance of profit 167*l.* 18*s.* 4*d.* This profit, though not as large as it ought to have been, had the stock been favourable for the experiment, far exceeds what could be made of the same quantity of food by fattening cattle. Were the two quarts to be added which on a moderate computation might be expected, the gain would then be £.207 16*s.* 4*d.* The trifling quantity of land from which the cattle were supported, is a most important consideration. One half of their food is applicable to no other purpose, and is equally employed in carrying on the system of a corn farm. I have found oil cake of the utmost advantage to my dairy, promoting milk, and contributing greatly to keep the milch cows in condition. The best method of using it, is to grind it to a powder, and to mix it in layers and boil it with the chaff: half the quantity in this way answers better than as much more given in the cake, besides the saving of 2*d.* a day on each beast. This I was not aware of on my first trial. The oil cake adds considerably to the quantity and richness of the milk without affecting its flavour. The refuse corn was likewise ground and boiled: it is charged also at 1*d.* per pound. I make use of inferior barly to great advantage. A change of food is much to the advantage of the deary. Potatoes steamed would answer admirably; but near towns they are too expensive.

The milk was better in quality than usual.

By repeated trials it was found that 7 quarts of strippings, wine measure, gave a pound of butter, while 8 quarts of a mixture of the whole milk was required to produce the same weight. Contrast this with milk produced from the feeding of grains, 20 quarts of which will scarce afford a pound of butter.

Produce of butter usually had from milk.

The Agricultural Report of Lancashire, treating on the milk in the neighbourhood of Liverpool and Manchester, states 18 quarts with a hand churn, and 14 or 15 with a horse churn. In a paper published by the Bath Society, 12 quarts are said to give a pound of butter: but wether ale or wine measure, is no specified. A friend of mine, who feeds his milch cows principally on hay, finds 16 wine quarts will not yield more than 17 ounces of butter, and this upon repeated trials.

The milch cows, treated according to my new plan, have been in excellent order both seasons, and are allowed to be superior to any in the neighbourhood.

Cole seed I have found to be the most profitable of all green crops for milk; and it possesses the further advantage of standing till other green food is ready to supply its place.

Comparative statement.

To ascertain the benefit and utility of a supply of milk, both to the consumer and the public will be best done by comparison.

To prove this, let us contrast the price of milk with other articles of prime necessity, and consider how far it affords a greater produce from a less consumption of food.

I cannot here omit observing, at a moment when Great Britain can hope for no further supply of grain from the continent, and must look for and depend on her own resources for feeding her population, every mean by which the quantity of victuals can be augmented, is an object of great public concern.

General numerical deduction.

Each milch cow, yielding 6 quarts of milk per day, furnishes, in the period of 200 days, 2,400 pounds of milk, or 171 stone of 14 pounds. equal to twice her weight, supposing her in a state fit for killing, with a third less food, and at one half less expense. The milk costs £.10; whilst the same weight of butchers meat at 6*d*. per pound would amount to £.60.

Taking the scale of comparison with bread, we shall find a Winchester bushel of wheat of the usual weight of 4 stone and 4½ *lb.* when manufactured into flour of three sorts yields:

Of first flour	-	2 st.	9 lb.
Of second	-	0	7 lb.
Of third	-	0	7 lb.
		3	9 lb.
Lost by bran, &c.	-	0	9½lb.

The present cost is 10*s.* 3*d.* 2,400*lb.* of the three sorts of flour will cost £.23 3*s.* 9*d.* To make it into bread, allow 1*s.* per bushel, which makes the cost of bread £.26 10*s.* 9*d.* or something more than 2½*d.* per *lb.* exceeding twice the price of the same weight of milk. To furnish 2,400*lbs.* of bread, requires 47 bushels, or the average produce of two acres of wheat.

Three acres of green food supplied 30 milch cows with 2 stone each of green food for 200 days. Two stone of hay each for the same period would have required 75 acres of hay. Chaff can scarcely be considered as of any value beyond the manure it would make, which shows the profit of keeping milch cows in all corn farms.

Certificates of the quantities of milk sold and money received accompany this.

If the Society of Arts, &c. think the experiment worthy their notice and approbation, I shall be highly flattered. At all events, I trust they will accept it as a small tribute of respect and gratitude for the many favours conferred upon their

Obedient and very humble Servant,

J. C. CURWEN.

Workington Hall, April 18, 1800.
To Dr. C. TAYLOR, Secretary.

VIII.

Some Account of the Manufacture of Forged Iron Vessels, at Fromont. *By* M. CH. HERSART.*

Fabrication of vessels of forged iron, for culinary and other uses.

THE operations of forging vessels of cast iron may be divided into three distinct parts: 1st, the method of forging the plates; 2d, that of forging the cake or parcel; 3rd, the cold hammering Of those we shall speak in the order here mentioned, which is likewise the order of fabrication.

To Forge the Plates.

The iron for this manufactory must be very soft and malleable. It has usually the form of bars, ten or twelve feet long; each bar having the form of a long truncated square pyramid. This form is necessary in order to obtain plates of different diameter. The small base is a square of ten lines, or twelfth of an inch, and the greater eighteen lines.

The assistant puts one of these bars in the fire, and when the heated part is ignited, the master forgeman carries it to the small tilting hammer, which is not different from those used in drawing out steel bars. He places the bar on the anvil, not upon one of its faces, but on an edge, as, in this position, the iron is less subject to crack. According to the size of the plate intended to be hammered out, the workman strikes a greater or less portion of the bar, presenting it in all situations to the hammer, in order that the plate may obtain a circular form. Between the plate and the bar itself, he fashions a small neck to facilitate its separation. In this manner, the workman continues to forge the plate on both its faces as long as the heat allows, after which he carries the bar to the anvil, and applies a cold chissel to the neck, upon which his assistant strikes in order to separate the plate from the bar. This last is then returned to the fire, in order to continue the operation in making a second plate. Sometimes, but this is only when the plates are small, the workmen make three at once.

When a sufficient number of plates has been thus fabricated, as they are of different sizes, namely, from three or four inches diameter

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* Journal des Mines, No. 112.

Fabrication of vessels of forged iron for culinary and other uses.

diameter to a foot, the workman disposes them in parcels, of which each contains four of equal dimensions, and then carries one of them to the hearth of the furnace, where the assistant takes them in the large tongs, Fig. 1, Pl. VI, and puts them into the fire, taking care to change their position often; and when the brass is red hot, the master workman, who holds a small pair of tongs in each hand, carries it under the tilting hammer, after having spread charcoal powder between the plates, to prevent their welding together. The two pair of small tongs have the form of Fig. 2, and are used to give a circular motion to the parcel, and to keep it on the anvil When he has finished hammering it, he changes the order of the four plates, and in making this change, he is careful to take notice whether any of them have cracked; and where he perceives any crack, he applies the cold chizel, or a wedge to the place on which the assistant gives a blow.

After having changed the situation of the plates in such a manner that the two outside plates become the interior ones, he places this parcel on the hearth, and takes another set, which the assistant has caused to be heated, and he subjects this to the same operation of the hammer. In this manner the process is conducted until the required dimensions are obtained, namely, after five or six heatings. He then places the plates on the ground to cool; and when cold, he cuts them circularly one at a time, with the large hand-shears, Fig. 3.

This being done, each face of the plate is severally covered with a mixture, formed of the oxide of lead and oxide of tin, pulverized and mixed with a little water; or, instead of this mixture, clay, diluted in water, may be used, as I have seen practised. Either of these will prevent the plates from welding together, and for that purpose it is that they were applied.

Forging the Cake.

The workman takes seven plates of the same size, coated as before described, with the oxide of lead and tin, and he places them upon each other. These seven being placed on two others of larger size, constitute what is called a cake, which is put into the fire by means of large tongs, not differing from the former, except in the mouth, or claws, which are rather higher and curved, as is seen in Fig. 4.

Fabrication of vessels of forged iron for culinary and other

When the cake is red hot, the assistant, who always has the management of the fire, takes it to the edge of the furnace, where the master workman bends the two large plates in one part, and takes up the cake with the tongs already mentioned, Fig. 2, when he carries it to anvil of the small forge hammer, in order to bend the edge of the two great plates entirely round. The difference between the diameter of the great and small plates, is about two inches: when this is done, he puts the cake again into the fire; and when red hot, he carries to a smaller caking hammer and that used before, but fixed and moved in the same manner. The anvil is a rectangular parallepipedon, which rises above the ground not more than one foot; and it has three pieces of iron bended to a right angle, at the height of the angle, which affords three branches converging towards the anvil, and serving to facilitate the operation of moving the cake during the work next to be described. See the plan and elevation traced, Fig. 10.

The workman being seated before his hammer, takes the cake with two small pair of tongs, and gives it a continual circular motion: during this commencement of the work, he hammers it only on the edge, after which he ignites it, he again carries it to the same hammer, first wetting the edge of the plates to diminish the heat which would only incommode him. By this second forging, he carries his stroke nearer to the center, still continuing the circular motion. By repeating the same operation as far as for eight times, continually approaching the center, the edge rises every time, and the assemblage of plates become more and more hollow. Accordingly, as this figure encreases, he finds it necessary to change his tongs for others, which differs from the first in the elevation of one of the jaws, and the extremity of the handle, Fig 5. After seven or eight ignitions, he carries the cake to a kind of anvil, the form of a figure 6, where he holds it with small tongs, Fig. 7, in order to complete the sides, which is done by the workmen hammering in succession, the hammer of the assistant being heavy and double-handed, when this is upon two at once. It is speedily done, and followed by another nearly similar on the bottom of the vessel, by a second hammer, placed near the first, striking on a kind of square anvil. Young girls, afterwards, are employed in scraping the bottom with an iron rod, Fig. 9. One foot and

Fabrication of vessels of forged iron for culinary and other uses.

and a half in length, terminating at one of its ends in a flattened small termination of steel. After this is done, the workman takes three vessels, one after the other, and presents them under a third hammer, placed near the two first, and moved like them by the same arbor, which carries a small tripping wheel, moved by water. The vessel is placed on the anvil, so that the hammer, which is pointed at its striking extremity, enters into its cavity. The workman holds the vessel, and shifts its position with his hands and knees. Every stroke of the hammer leaves a slight cavity of the size of a pea, which forms different designs, according to the motion which the workman gives to the vessel. These outlines are not made for the sake of beauty, but to give strength and firmness to the vessel by hammer hardening it. The young girls, afterwards, take the vessels and scrape the interior sides, as was done with the bottom; and lastly, the workmen, on two kinds of anvils, the one plain and circular for the bottom, and the other semi cylindric for the sides, completes their figure with a wooden mallet. Small cracks sometimes appear in the vessel, which the workman close, and the matter is suffered to cool; after which, the cake, which now has the form of a tuencated cone, is carried against a piece of iron bended two ways, Fig. 8, and drove into the wooden block which supports the gudgeon of the arbor of the hammers. This doubly recurved iron serves to retain the cake which enters under it, and by that means allows the small tongs, Fig. 7, to raise up the edges of the two great plates, which, in part, covered the seven small ones. This being done, the vessels, or hammered pieces, are taken out from within each other. The first is always perforated on account of the immediate purchase of the hammer, and that of the air, which, partly converted into scales, fall out by the immediate action of the hammer. As these vessels, when taken out, are more or less bended, the assistant sets them to right by a few strokes of the hammer, after which the master workman cuts their edges with the shears.

Cold Hammering and finishing.

After the vessels are cut round, they are delivered to another workman, who takes them to his separate shop to finish. His first operation is to set the conical surface fair by means of a small hammer, upon a proper tool. The workman holds the vessel with his right hand with his small tongs, 7; and with his left hand, without tongs, taking care to turn it round continually. Sometimes he performs this operation with a stroke of the hammer; and the complete finish is made by cutting the edges with scissars, similar to those before described.

The furnace made use of is a simple forge furnace, and the fuel is charcoal of fir, excited by wooden bellows.

IX.

Description of an Astronomical Circle, and some Remarks on the Construction of Circular Instruments. By JOHN POND, *Esq.*[*]

From the Philos. Transactions, 1806.

Astronomical circle of two feet in diameter,

THE observations which accompany this paper were made at Westbury in Somersetshire, in the years 1800 and 1801, with an astronomical circle of two feet and a half diameter, constructed by Mr. Throughton, and considered by him as one of the best divided instruments he had ever made; a drawing of it, with a short description, is anexed to the observations. (Plate VII.)

—applied to determine the declinations of some principal stars.

When this instument came into my possession, I thought I could not employ it in a more advantageous manner, than in endeavouring to determine the declinations of some of the principal fixed stars[†]. The various catalogues differed so much from each other, and such doubt existed as to the accuracy of those which were thought most perfect, that the declinations of few stars could be considered as sufficiently well ascertained for the more accurated purposes of astronomy.

The

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[*] The title of this Paper, in the Transactions, is, " On the Declinations of the principal fixed stars, with a description," &c but on account of the extent of the tables of observations, I must refer the astronomical reader to the Transactions for them. The number of stars, observed at Westbury, were 29; and of those compared with the Greenwhich observations, 37.—The plate could not be finished till the next Number.

[†] At that time Dr. Maskclyne's late Catalogue was not published.

Method of observing, &c.

The advantages that have resulted from the excellent method persued at Greenwich, of observing constantly the transits of a few stars, to obtain accurately their right ascensions, induced me to follow the same method for determining their declinations; and for a considerable period I constantly observed them on the meridian, whenever they passed at a convenient hour; usually reversing the instrument in azimuth at the end of every day's observation; never considering any observation as complete that had not its corresponding one in a short interval of time. When this circumstance is not attended to, I think, a great part of the advantage arising from the circular construction is lost.

Accuracy of the observations.

Greatest error of collimation.

The observations themselves will show, if they have been made with the requisite care and attention to merit the notice of astronomers; for it is one of the many advantages of circular instruments, that, from the observations made with them, we may infer with great precision not only the mean probable error, but likewise the greatest possible error to which they are liable. From a careful comparison of the errors of collimation, as deduced from different stars, I concluded that the greatest possible error was 2″,5, and the mean error about 1″; and by a comparison with other observations with similar instruments, it will be seen that this supposition was well founded, since nearly the same quantities are deduced by another method to be considered hereafter.

Polar distances

The polar distances are annexed to each observation: a method which I borrowed from Mr. Wollaston, and which is rendered very easy by employing his useful tables calculated for that purpose. This practice of reducing every day's observations cannot be too much recommended, as the labour of calculating accumulated observations is thus rendered unnecessary.

Comparison with other observations.

—namely, at Greenwich, at Armagh, at Palermo, and

When I had deduced the declinations of these stars from my own observations, continued long enough to divest them of all error, except that arrising from defect in the divisions of the instrument. I was desirous of comparing them with the observations made by the others; and I have subjoined a comparison of them with all those which I could procure, that seemed entitled to confidence. In the first column are the observations made at Greenwich, as published in 1802 by the Astronomer Royal; the second column, contains a cata-

logue

by the author at Westbury.

logue deduced from some observations made at Armagh with a very large equatorial instrument, constructed by Mr. Troughton; in the third column, are the observations of Mr. Piazzi, of Palermo; and in the fourth, those made at Westbury. All the above mentioned observations are arranged in the order of their polar distances, and the positive deviations separated from the negative; that the cause of error in any of the instruments, may be the more easily detected, as likewise any mistake in the assumed latitudes of the respective places of observation.

General catalogue.

A general catalogue is then added; which is deduced, by taking the mean, generally of the above four; but in some places, a few detached observations that I have accidentally procured of other circular instruments, have been included. The utility of this investigation is not merely confined to the determination of the polar distances of the stars; as, besides this, some valuable information on other points may be obtained, In the first place, upon examining the variations that appear in these observations a question naturally occurs, whether, by changing the assumed latitudes of the respective places of observation, a nearer coincidence might not be obtained? And I find, that to make the positive deviations equal to the negative, the following corrections should be applied to the co-latitudes;

Greenwich + 1″
Armagh + 1″,3
Palermo — 1″
Westbury — 0″,25.

New correction of the latitudes from the deviations of observations

This method of correcting the latitudes has, I believe, never been employed; but it seems reasonable to suppose, that, when thus corrected, they will be nearer the truth, than those determined by the usual method: for the same reason, that the declinations of the stars resulting from a general comparison, are more likely to be accurate, than if deduced from any one single set of observations; but if the Greenwich instrument should be affected with any errors independant of the divisions, in that case, we should be unable to infer any thing decisive, as to the latitude, by the above method. But from a comparison of the observations of γ Draconis, observed at Greenwich and Westbury, the latitude of Westbury being previously corrected by the above method, I am inclined to believe the latitude of Greenwich requires a very small correction, certainly not exceeding a second. The result I obtain by a very careful investigation by methods, entirely independent of the Greenwich quadrant, is 51°.28'.39",4.

The comparison of observ.

I consider this comparison as interesting likewise on another account; it is an object deserving of curiosity to examine the present state of our best astronomical instruments, and to ascertain what may reasonably be expected from them. The superiority of circular instruments is, I believe, too universally admitted, to render it probable that quadrants will ever again be substituted in their place. But the Greenwich quadrand is so intimately connected wih the history of astronomy, the observations that have been made with it, and the deductions from those observations, are of such infinite importance to the science, that every circumstance relating to it cannot fail of being interesting Now, when it is considered that this instrument has been in constant use for upwards of half a century, and that the center error, from constant friction, would, during this time, have a regular tendency to increase, it will not appear at all surprising, if the former accuracy of this instrument should be somewhat impaired. With a view, therefore, of ascertaining more correctly the present state of an instrument on which so much depends, I have exhibited in one view the polar distances as determined by circular instruments alone; the respective co-latitudes being previously corrected by the method above mentioned, and I have compared the mean result with the Greenwich Catalogue, that the nature and amount of the deviations may be seen, and if it be judged necessary, corrected. I should add, that by some observations of the sun at the winter solstice in 1800, the difference between the Greenwich quadrant and the circle was 10 or 12', the quadrant still giving the zenith distance too little.

shows the present state of instruments.

General Description of the Instrument.

Description of the author's circular instr. made by Troughton.

The anexed plate represents the circle in its vertical position. It was originally made to be used likewise as an equatorial instrument, a circumstance I need not have mentionned, but as an apology for slightness of its construction, which the artist, who made it, would not have recommended, had the instrument been intended for the vertical position only.

The

Decl. circle.

The declination circle, 30 inches in diameter, is composed of two complete circles; the conical radii of which are inserted at their bases in an axis about twelve inches long, leaving sufficient space between the limbs for a telescope $3\frac{1}{2}$ feet long, and an aperture of $2\frac{3}{4}$ inches, to pass between. The two circles are firmly united at their extreme borders by a great number of bars, which stand perpendicular between them; the whole of which will be readily understood by referring to the figure. The square frames, which appear as inscribed in the circle, were added to give additional firmness to the whole.

The circle is divided by fine lines into 5′ of a degree; and subdivided into single seconds by two micrometer microscopes, the principles and properties of which are now too well known to require any particular explanation.

Great improvement that the microscopes are capable of having their position varied by a circular motion of the diameter which connects them.

At the time these observations were made, the microscropes were firmly fixed opposite to the horizontal diameter; but when I considered that, by continuing the observations, the error of division would never be diminished, I suggested to Mr. Troughton the possibility of giving a circular motion to the microscopes; though I confess with very little hope, that the thing was really practicable in an instrument previously constructed on other principles. Mr. Troughton approved of the idea, and executed it in a very ingenious manner. His talents, as an artist, are too well known and too highly appreciated, to stand in need of any praise from me; yet I should consider myself as deficient in justice, if I did not endeavour to call the attention of the reader to the skill and ingenuity, which have been employed not only in this very important alteration, but in every contrivance that is peculiar to the instrument, which is the object of our present consideration.

These microscopes can now revolve about 60° from their horizontal position; and it is easy to comprehend, that, by this valuable improvement, all errors of division may be completely done away, without any of the manifest inconveniences of the French circle of repetition; which, though a very ingenious instrument, and admirably adapted to some particular operations, will, I think, never be adopted for general use in our observatories.

The plumb-line

The plumb-line, a very material part of this instrument, is suspended from a small hook at the top of the tube at the left hand of the figure. It passes through an angle, in which it rests in the same manner as the pivot of a transit instrument does on its support. At the lower end of the tube which protects it, a smaller tube is fixed at right angles, which contains microscopic glasses so contrived, that the image of a luminous point, like the disc of a planet, is formed on the plumb-line and bisected by it. Great attention should be given to the accurate bisection of this transparent point by the plumb-line *at the moment of observation.* It is absolutely essential in instruments of this construction, to consider the observation, as consisting in two bisections *at the same time*: the one of the star by the micrometer, the other of the plumb-line-point by the plumb-line. The least negligence in either of these bisections will render the observation unsuccessful.

Upright pillars and vertical axis, passing down in stone pedestal.

The two strong pillars, which support the axis of the vertical circle, are firmly united at their bases to a cross bar; to which also the long vertical axis is affixed, and which may be considered as forming one piece with them. The stone pedestal is hollow, and contains a brass conical socket, firmly fastened to the stone, and reaching almost to the ground. This socket receives the vertical axis, and supports the whole weight of the moveable part of the instrument, which revolves on an obtuse point of the bottom; the upper part of this vertical axis is kept steady in a right angle, having two springs opposite the points of contact, which press it against its bearings, and it thus turns in these four points of contact with a very pleasant and steady motion.

Adjustment of the vertical axis.

The bar, in which the vertical axis is thus centered, is acted on by two adjusting screws in directions at right angles, and perfectly independent of each other. By these motions, the axis may be set as truly perpendicular, as by the usual method of the tripod with feet screws, which could not in this case have been employed.

The frame to which this apparatus is attached, is fixed to the corners of the hexagonal stone, by the conical tubes; between which and the stone, the azimuth circle (which forms one piece with the vertical axis) turns freely. The azimuth circle of two feet diameter, consists of eight conical tubes, inserted in the vertical axis, and which are united at their ends by the circular limb; this is divided and read off exactly in a similar manner to the other circle.

A level

—and of the horizontal axis.

A level remains constantly suspended on the horizontal axis, which is verified in the same manner as in a transit instrument. There are forcing screws for this purpose, which pass through the bar on which the vertical columns stand, and these by pressing against the long axis, produce a small change in the inclination of the upper part of the instrument, without altering the position of the azimuth circle or its axis.

The application of the plumb-line, as already described, is peculiar to the instruments made by Mr. Troughton: it regards the vertical axis rather than any other part, and is, in fact, exactly analogous to the usual verification of a zenith sector.

On the adjustment of circular instruments

During the period in which I was engaged in making observation with circular instruments, I was led to consider the advantages and inconveniences of the usual method of adjusting them; and it appeared to me, that the essential part of their construction, which relates to their adjustment, was capable of being improved.

In order to render the nature of the improvement, which I wish to propose, more intelligible, I ought previously to remark, that there are, at present in use, two modes of adjusting these instruments, which are founded on different principles.

Two methods now in use. Two points on the circle are brought into a vertical line by means of the plumb line, and the index is then brought to zero.

In the one, two points are taken on the limb of the circle; and when these are brought into a given position, by means of a plumb-line passing over them, the microscope or index is made to coincide with the zero point of the divisions: by this method, the error in collimation remains constant; and if the adjustment is by any accident deranged, it can easily be rectified, and there will be no absolute necessity for frequently reversing the instrument: so that this method seems well adapted for large instruments, particularly if placed on stone piers. But it is liable to this defect, that the adjustment cannot be examined at the moment of observation; and if any change should take place in the general position of the frame work, the observation will be erroneous without the means of detection. It was probably to avoid this inconvenience, that Mr. Troughton, in most of his instruments, particularly if they were intended to move freely in azimuth, has preferred the other method.

Objection. The frame work may vary, and render the observation erroneous.

Or. 2. The plumb-line being attached to the support of

In this case, the plumb-line is attached to one of the pillars, which support the microscopes in the way above described; and it has no reference to any fixed points or divisions on the limb of the circle, but only insures a similarity of position in the index, for each position of the instrument; and, provided that the plumb-lime apparatus was free from all danger of derangement, this would be sufficient. This verification may be rendered perhaps more intelligible, by considering that a circular instrument, in whatever manner its vertical axis be placed, indicates by a double observation, the angle which the object makes with the axis, round which the whole instrument has revolved in passing from one position to the other. For let Pp be the axis, Tx the telescope x in one position; it is evident, that in turning the instrument half round, ty will then be the position t of the telescope; Px being equal to Py. The arc xy, which the telescope passes through to regain its former position, is the quantity really given by the instrument; and if the axis Pp be vertical, half this quantity is the true zenith distance of the object. Now the intention of Mr. Troughton's verification is to insure a vertical position to the axis Pp.

the miscroscopes, insures their position, and each observation is made with a corresponding one, after reversing the instrumt. in azimuth.

For instruments which rest on moveable pillars, and turn freely in azimuth, this method is much to be preferred; but it is not without a considerable defect: for, if by any derangement in the plumb line apparatus, the error in collimation be changed, it cannot be restored with certainty to its former position; so that sometimes a very valuable series of observations may be lost, for want of a corresponding one to compare with it. The mode which I propose to adopt to remedy these inconveniences, will enable us to combine all the advantages of the two methods above described: it is extremely simple in its principle, and easy of execution, for it merely consists in uniting on the same plumb-line the two principles already explained.

Objection, that the corresponding observation cannot be had if the plumb-line should be deranged.

Two very fine holes should be made in the farther limb of the circle,, and two lenses firmly fixed opposite to them, in the other, which should each form an optical image of its corresponding dot or hole, in the tube through which the plumb-line passes.* It will be best, if these dots are made exactly

Union of both methods in one by the author.

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* As these transparent dots are intended to be bisected by the plumb-line, they must be capable of the necessary adjustments, both for distinct vision, and for placing them in an exact diameter.

It may be found more convenient in practice to arrange the whole apparatus in sliding tubes, but in whatever way the contrivance be executed, the points should ultimately be fixed as firmly as the divisions of the instruments.

exactly in a diameter, as they may then be used in two positions. Beneath these should be formed the image of a luminous point, according to Mr. Troughton's present method, by an apparatus attached to the plumb-line tube; when the two points on the circle move away, by the necessary operation in observing, the lower point will remain stationary, and indicate any change of position in the whole instrument, if such should accidentally take place, and which by the other method alone would have passed unnoticed.

The second is most deserving of confidence.

The contrivance above described was executed for me at my request by Mr. Troughton, and is represented in the plate; but by some accident a part of the apparatus was broken in putting it together, so that I never was able to use it. As each apparatus for this adjustement is quite independent of the other, no possible inconvenience can attend their application, as either may be employed alone, at the option of the observer. But as any verification requiring many bisections is objectionable, I would in general certainly prefer Mr. Troughton's method, and only have recourse to the other, when there was reason to suspect that some alteration had taken place to render it necessary.

Errors of division in the instrument,

One more circumstance respecting the instrument remains to be noticed: when the divisions were first examined by opposite readings, 1",25 was the greatest possible error which was to be apprehended, and 0",7 the mean error; but in its journey it seemed to have suffered some very small derangement in its form: this was discernible both from examining the opposite readings; and by deducing the error of collimation by zenith stars, and comparing it with that found by an horizontal object, there was constantly perceived a difference of 3" between the error of collimation deduced from γ Draconis and by an horizontal object; and this quantity was very uniformly distributed through the intermediate arc. In what particular manner the observations would be affected by this derangement I willl not venture to decide, but I think it most likely that it has only rendered the instrument rather less accurate than it was originally, as is above stated. I have before observed the great advantage the circle possesses of showing the amount of its own errors. These may be determined with great certainty by examining the errors of collimation as deduced

--determinable with great certainty from the stars.

ced from different stars. This method is founded upon the supposition that half the difference of the two extreme quantities is the greatest error of division, which has in this case influenced each result in an opposite direction. For instance, let us suppose the errors of division never to exceed 2", but occasionally to amount to that quantity, on several parts of the circle; it will then sometimes occur that each index will give 2" too much in one position of the instrument, and 2" too little in the other; there will then appear a difference of 4" in the error of collimation; but the observations in these extreme cases will not on that account be the less to be depended on; on the contrary, the probability is in favour of their superior accuracy.

Nor, on the other hand, will those observations which give the mean error of collimation deserve greater confidence than the rest, since it is evident that some of them may be, and most probably are, affected with the greatest possible error; for we suppose the most erroneous observation to arise from the greatest error of division occurring on each of the four arcs in the same sense, that is all *plus* or all *minus*; nevertheless, the observation thus erroneous, will give the mean error of collimation.

The great accuracy of observing by circular instrument is to be expected from the revolving microscopes.

By an attentive consideration of these circumstances, corrections might perhaps be obtained which would somewhat diminish the probability of error. But it is to the principle of the revolving microscopes, that in the future construction of instruments we should look for perfection. In the French circle of repetition, too great a sacrifice is made to the supposed advantage of reading off a great number of observations at once. Our best instruments are too well constructed to stand in need of this contrivance, as the divisions on a two-feet circle are read off with precision to a single second. The errors of simple division alone are those which continued observations have no tendency to diminish; these, by making the microscopes revolve, may be completely done away. An instrument thus constructed would be well adapted to detect small motions in the fixed stars which hitherto have escaped notice, or such as are but imperfectly known; for we cannot reasonably conclude that what is termed the proper motion of a star, is so uniform and constant, that being once determined, it will remain always the same.

The method of repetition does not correct the errors of division.

X.

On the Utility of the Lichen of Iceland as Food. By Professor PROUST. *Abridged from a Memoir in the Journal de Physique for August,* 1806.

Enquiry whether the lichen may not afford a resource in times of famine.

THE Professor begins his memoir by remarking, that the severe pressure of famine, and the diseases which follow in its train, were such as, a few years ago, directed the attention of every thinking man to the means of affording subsistence to the poor. And, under this interesting head of enquiry, he asks, whether the lichens, of which numberless species cover the rocks throughout Spain, and which constitute a large part of the food of the Laplander and people of Iceland, do not promise advantages well deserving investigation.

The Iceland lichen grows in Spain, and elsewhere in southern Europe.

Don Mariano La Gasca has discovered, in the environs of the monastery of Harvas, the very lichen of which the Icelanders prepare a food, which travellers affirm to be as substantial as wheaten bread. This monastery is situated at a considerable elevation in the mountains which separate the province of Leon from Asturias. It is also found in great abundance in many parts of the latter province. It was before known to grow in many places of Europe, but has been principally spoken of as an article of medicine; concerning which our author speaks in a general way, and without any marks of approbation, in the course of two pages, through which it is needless to follow him.

It is of little value for dyeing.

When Mr. Proust received the lichen from La Gasca, he found reason to examine it rather as an article of food than a dyeing drug; being induced to do so by the reports of travellers, collected in the Apparatus Medicaminum of Murray, which are the following:

Reports of travellers concerning itstus as food in he north.

Von Troil informs us that the Icelanders made excursions of a week or a fortnight to the districts which produce the Lichen, which they carry home and keep in sacks till the time of use, when they wash it and reduce it to flour. Olafer asserts that two measures of this powder are as nourishing as one measure of wheat flour. After soaking it in water for a day to take away its bitterness, they boil it with whey till it is converted into jelly. They eat it either hot or cold, with an additional portion of whey or milk.

According to Benzelius, the Laplanders boil the lichen in one or two waters and throw away the decoction. They afterwards wash it in cold water and boil it in milk, after having crushed it. This soup is seasoned for use with salt. The author shows that a part of the nutriment is thrown away along with the decoction.

Some Swedish botanists, who travelled in Lapland during the summer of 1788, when the north of Germany and the west of Bothnia, were afflicted with a cruel famine, subsisted upon it for a fortnight. They soaked it all night in hot water, and in the morning they boiled it with milk.

It is very nourishing to animals.

Scopoli informs us that, in Carniola, there is no food known which fattens animals so speedily as this lichen. Lean horses and oxen are taken to the places where it abounds, and in less than four weeks they become very hearty and fat.

According to Pallas, the people of the northern part of Asiatic Nessia support themselves upon a lichen when their other provisions fall short.

After these introductory particulars our author proceeds to give the result of his observations and experiments.

Method of preparing it previous to washing.

Cold water takes away its bitterness,

The lichen is cleaned by picking out the mosses and fragments of wood, and washing away any earth which may lie among its roots, by rubbing it with the hands under water. A very short time of immersion in cold water restores the colour and humidity of this vegetable, and more than doubles its weight. In order that the water may extract its bitterness, it is necessary to crush or divide its parts by cutting or pounding In this state water extracts, in the course of three hours, a bitter and slightly yellow juice, not absolutely disagreeable, but very supportable when the plant is prepared by simple boiling, without maceration. The extraction of the bitter principle by cold water diminishes the weight about three parts in the hundred. Hot water takes out the bitterness more speedily, but at the same time extracts about an equal quantity of the nutriment. But this trifling loss is compensated by the speed with which the effect is produced. The bitter principle is an extractive matter, which strikes a brown with iron, and is used for that purpose by the Icelanders. It is not, however, so good as to be preferable in the European dye-house.

but not its nutritive principles.

Such infusions as are here mentioned would extract the most useful parts from vegetables in general, particularly gums and sugars; but the lichen sustains no considerable loss, because its nourishing and soluble parts are very different from sugar, gum, or even farina.

It requires a slight boiling, and gains twice its weight.

One quarter of an hours boiling in water is sufficient to cook the lichen, and render it as tender for use as can be wished; at the same time that it extracts the soluble principle. After the boiling, one pound of the lichen from its dry state, three pounds of the plant, fit to be served up as a vegetable in the solid state, are obtained. It is remarkable that the boiled lichen, when pressed in a cloth, to expel the superfluous water, recovers its first volume as readily as a sponge. So far from resembling those vegetables which have a ligneous structure, which requires considerable boiling, it has the elasticity of some champinions, and eats like the very tender cartillages of animals. The lichen has not, however, any analogy to animal substances.

When boiled and dried scalding water instantly swells it out, and renders it eatable.

The boiled lichen when dry, and preserved in that state, resumes its elasticity in an instant, and becomes fit for the table by pouring boiling water upon it. Fresh water, or sea water, are equally applicable to this object. Some persons, who have eaten it at the author's table, and knew by experience what an invaluable resource fresh vegetables are in long voyages, remarked, that a provision of boiled lichen would afford a fresh sallad, no less agreeable than advantageous to the health, under circumstances of this nature. The author thinks that this sallad, which, without any consumption of fresh water, is itself as fresh as if it came out of the garden, must be of advantage against the scurvy. Cold water answers the purpose here mentioned as well as hot, but is not quite so speedy; so that the preparation of this sallad does not, in strictness, require any consumption of fuel.

Sea water may be used.

It is an agreeable food.

Professor P. speaks with great commendation of this vegetable, when seasoned and served up under roast meat, and also as a sallad. All his friends approved of it; and the consumption of his kitchen was incomparably greater than that of his laboratory. He remarks, that a slight boiling gives it all the tenderness it is capable of, but does not take out its bitterness, which requires a little longer time: but he remarks, that the bitterness is not at all disagreeable; and that if its effects be aperient, as Scopoli affirms, it would agree with many constitutions.

According to the judgment of several Americans, the boiled lichen particularly resembles the fucus, which is called luche at Lima, of which so great a consumption is made along the whole coast of Peru and Chili.

As a pound of lichen affords three pounds of boiled vegetable, and these, when dried, are reduced to two-thirds of a pound, it clearly follows, that two-thirds of this food, when taken, consist of water. Mr. P. anticipates this as an objection which might probably be made against its nutritious quality. And to this he replies, that it is probable that water may be among the substances upon which the digestive faculties act, and which, by its decomposition, may serve as food. He refers, in support of his arguments, to other articles in common use, which are liable to the same objection, such as boiled potatoes; and he asks whether, since a dozen of the whites of eggs really contain only one ounce of dried albumen, we are authorized to conclude that a man, who should have dined upon this dozen in an omlet, had not made a solid and satisfactory meal.

Observation to shew that it constitutes a solid nutriment, notwithstanding the great quantity of water absorbed.

The former part of the Professor's Memoir was confined to the domestic uses of the lichen. In his second part, he treats of its chemical examination.

Chemical examination of the lichen.

Many of our domestic plants are unfit to form a component part of soups and other liquid foods; but the lichen is eminently qualified for this use, its decoction being charged with nutritious matter. This soluble substance might, at first consideration, be classed with the gums; but it differs so considerably from these that the author thinks it forms a particular species, entitled to attentive examination.

It affords a nourishing matter to soups

It was before observed that the lichen loses one-third of its weight by boiling. But more strictly speaking, he informs us, that one hundred parts of the lichen grossly powdered, afford, by infusion in cold water, three parts of the extractive bitter principle; and after treatment with boiling water, the undissolved residue amounts to sixty-four parts when dried. Consequently the quintal of dry lichen consists of

Statement of its immediate parts as separated by water.

Fleshy, or pulpy part	64
Bitter part	3
Unknown part	33
	100

A new species of gum soluble in boiling water but not in cold.

The thirty-three parts consist of a nutritious matter, not soluble in cold water. The bitter principle is therefore separable from the rest by cold infusion of the pounded plant; and the Icelanders follow a more saving method than the Laplanders, who throw away the decoction.

Experiment for making the thick milk of the Icelanders with lichen.

The Professor boiled the farina of washed lichen with milk, until it appeared to be sufficiently cooked; and this, when seasoned with pepper and salt, was entitled to a comparison with rice, or millet, boiled in milk. It has a greenish colour, and is a little acid: but not more incompatible with habitual use among the poor than many other country foods, which those who are not accustomed to them, would by no means consider as dainties.

Another dish was made by seasoning the preceding milk and lichen with yolk of egg and sugar. It was much more pleasant, but certainly not superior in its nutritious qualities.

Great strength of its galatinous decotion.

The decoction of this plant is of a light yellow color, and has a slightly bitter taste. Its gelatinous quality is so predominant that one pound of dry lichen affords, by boiling, about eight pounds of liquid, which congeals by cooling. And as no more than one-third of the weight of the lichen enters into the decoction, the jelly itself is formed of one part of this peculiar gum and twenty-three parts of water. Professor P. considers this as the only vegetable matter capable of rendering so large a proportion of water gelatinous.

The water separates by standing.

The jelly of lichen exhibits some peculiar properties. When left to stand for some days, the water is seen to separate at the edges, however cool the place may be where it is kept. If the plate be inclined in different directions, the jelly breaks with a facility much greater than is seen in animal jellies, or even those of fruits; and in the cracks the quantity of water increases, as if there were but little attraction between that fluid and the jelly.

It is not at all viscid.

During the boiling of the jelly this principle forms, at the surface, transparent bulbs, which are renewed continually when skimmed off; but these bulbs, when again added to the fluid, do not dissolve unless it be boiling; a fact which confirms the opinion that this species of mucilage is less soluble than any with which we are acquainted in the vegetable kingdom. Unlike every one of the known gums or mucilages, this jelly, when concentrated, is not viscid, either cold or hot, and it may therefore be doubted whether it would be useful in the arts, unless for calico printing; a point which deserves to be examined*. From this want of viscidity it happens that the jelly, when dried upon plates, divides into transparent angular and brittle fragments of a deep red colour. The animal or vegetable jellies differ extremely from it in this respect, because the viscidity, which connects their parts, prevents their separating while drying.

Habitude of the dried gum with water.

If the dried gum of the lichen be thrown into hot or cold water it does not dissolve, but it softens and swells up, without becoming viscid or tenacious; but it deposits very speedily all its extractive matter, and consequently its bitterness. This property affords a means of purifying it in case it should hereafter be found of service in the arts.

The green is precipitable by galls, but the precipitate is soluble in hot water.

The infusion of galls, which has no action upon the known gums, instantly precipitates the jelly of the lichen, and affords a white mass, as with animal jellies; but there is this difference, that the new combination dissolves in hot water, and separates by cooling. In this fact we have a character of the mucilage of lichen, which seems to assimulate it with that of animal matters; but the following experiments decide otherwise.

Products by destructive distillation.

The gum of lichen, heated in a retort, is destroyed without being softened. Like gum arabic it leaves only 23 or 24 hundredths of charcoal. Its products do not differ from those of gum or starch; that is to say, they consist in water and vinegar of the same odour; but the oil appeared to be much more abundant. Potash separates from this vinegar only a few atoms of ammonia.

Nitric acid produces the axalate.

The nitric acid readily converts it into very white oxalic acid, leaving no tallow, or yellow bitter principle, in the residue. Lichen, boiled and dried, affords no more than 21 or 22 hundredth parts of coal.

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* The jelly of lichens was examined eight or nine years ago by Lord Dundonald, and offered for this purpose to the calico printers. N.

The nitric acid dissolves the boiled lichen with great facility; an effect which is not commonly seen with the ligneous vegetables. The product is oxalic acid and oxalate of lime, augmented, as seemed to the professor, by a foreign earth. The residue contains the yellow bitter principle in a very small quantity.

Habitude with potash.

Potash converts boiled lichen into a gelatinous pulp, similar to that which is afforded by farina in like circumstances. These facts appear to show, that the fleshy part of this plant is an indurated gum, less oxigenated perhaps than those which are soluble. The jelly of lichen may be used as food. The author made a very good blanc-manger, by adding a small quantity of flour, with sugar, and afterwards some milk, or emulsion of almonds. The author here makes a remark concerning the necessity of condiments or seasoning, to give flavour to this jelly; and takes notice, that the same necessity exists with regard to all gelatinous foods, whether animal or vegetable. Starch, as he remarks, is the basis of bread, and glue of soup; neither of which would be acceptable to the palate, or supportable by the stomach, without some stimulating ingredients to season them. The analogy of lichen to starch, in its want of solubility in cold water, is opposed by the difference, that it has no adhesion when dissolved; but he thinks it would be interesting to treat the lichen by an appropriate fermentation, to see whether this ferment would not separate it like starch. He likewise suggests the probable advantages to be derived from an examination of the several species of the vast family of the lichens.

Blanc monge, &c.

Whether the gummy matter would afford starch by fermentation.

XI. On

XI.

On the breeding and feeding Game Cocks. From Sir JOHN SINCLAIR'S *Collection of papers on Atheletic Exercises.**

Questions proposed.

Parentage of game cocks.

1. DOES the superiority of game cocks depend upon parentage? Which is of most importance, the male or the female? Is it of any consequence that the cock should arrive rather gradually at maturity? Is there a great difference, in point of strength and constitution, in game cocks of the same parentage? Do you prefer great or small bones?

Feeding.

2. When do you begin to feed the young cocks? What diet and drink do you give them, and what is the process by which they are brought to the greatest possible height of strength and spirit?

Longevity.

3. When the game cocks are thus trained, how long do the effects thereof last? Are they temporary or permanent? Do game cocks thus trained live shorter or longer than others of the same species?

Medicines.

4. What drugs are given to fighting cocks immediately before the main begins? Is it not usual, by giving them saffron, (or some drug, which has the same effect with opium, as used among the Janisaries, or brandy among the French soldiery), to excite an unnatural and short-lived courage? What are the effects of such drugs? and how do they manage the feeding up to this point, so as to take advantage of this momentary excitement?

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* The moral effect of cock-fighting is, no doubt, a subject deserving to be considered; but concerning this, as the Selector of a philosophical article, I wish to be supposed to advance no opinion at present.—N.

The following interesting Letter was received from a Clergyman.

West Ham, March 28th, 1805.

Dear Sir,

Information respecting breeding, training, and management of game cocks.

I perceive that only on one part of your well directed Queries I am able to give you satisfaction, and that is, on what you would least expect from a D. D. and the sober vicar of a country parish: the subject to which I allude is cock-fighting. At the period of my childhood, when I ran wild, from ten to fifteen, I was a great cock-fighter, and though it is many years ago, I find my memory perfectly competent to even the minute narration of every fact.

But before I proceed, I will intrude a remark or two upon your preliminary observations. In all the theoretical part I completely coincide: indeed I was pleased to find so much harmony between your sentiments and those I lately transmitted to you, without the possibilit[illegible] any previous concert between us.

I do not even question your facts, but seem to differ a little with respect to some of the inferences. With respect to the South Sea islanders, and the difference between them and the English sailors, I doubt whether there was any superiority in the training of the former, which gave them the advantage. An English sailor is, perhaps the very perfection of agility in his own way*. I do not know that the human powers can go beyond it, in some instances, that I have seen with my own eyes; yet an English sailor, though he could probably *climb* a rope better, could not *dance* upon one, as I have seen the people at Sadler's Wells. The superiority, therefore, of the South Sea indians in wrestling, boxing, and rowing, I attribute merely to *practice*. It was also in their *own way* that Cooke's sailors contended with them. In a fair boxing-match, I have not a doubt but Mendoza or Humphries would have triumphed over at least twenty of them in succession. By the way, from what I have learned of amateurs, respecting these pugilists, no persons can lead more dissolute lives, except in the article of exercise. With this exception, that those among them who drink moderately (and moderation with them is free-living among other people) are the strongest.

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* An officer of a frigate who had been at the Sandwich Islands has declared, that our sailors stood no chance in boxing with the natives, who fight precisely in the English manner. A quarter-master, a very stout man, and a skilful boxer, indignant at seeing his companions knocked about with so little ceremony, determined to try a round or two with one of the stoutest of the natives, although stongly dissuaded from the attempt by his officers. The blood of the native islander being warmed by the opposition of a few minutes, he broke through all the guards of his antagonist, seized him by the thigh and shoulder, threw him up, and held him with extended arms over his head, for a minute, in token of triumph, and then dashed him on the deck with such violence as to fracture his skull. The gentlemen added, that he never saw men apparently possessed of such muscular strength. Our stoutest sailors appeared mere shrimps, compared with them. Their mode of life, constantly in vigorous action in the open air, and undebilated by the use of stimulating food or drink, may be considered as a perpetual state of training. Sir J. S.

Information respecting the breeding, training and management of game cocks.

On a subject where I am more at home, my observations will lead to the conclusion, that the simplest mode of living is the most conducive to bodily health and strength. Though very young when I pursued cock-fighting, from nice observation, which enabled me to judge of a good cock, and from a rational mode which I fell into of treating them, I hardly ever lost a battle, even against odds; but I will pursue the subject in your own order.

1st. There is not a doubt but that the sterling courage of an English game cock depends upon parentage. It is a maxim in the cockpit, that if a cock has, what they call a *spice* of the dunghill, though ever so remote, when he is galled by the spur he will run. I remember seeing a most famous cock, about eight years old, and who had in his time won forty battles, run at the last, when severely galled. A dunghill however fights harder for a round or two than a genuine game, whose courage is of a more temperate cast, and this very famous cock was an instance, who generally killed his antagonist with a stroke or two.

A true game-cock is, however, so well known by his marks, that sportsmen will rarely be mistaken. My mother has bought a clutch of chickens at the door, and I have selected from them one or two by my eye, which have proved incomparable. One of these chickens gained ten battles in one day, the last against on old cock, double his weight, and after mine, which was but a *stag* (that is one year old) had been cut down to the ground, and was *counting out*; that is, given up for dead.

Information respecting breeding, training, and management of game cocks.

Large bones are always preferred in cocks, and it is an excellence to stand high on their legs, for this gives them an advantage over those of a squat make.

2d. The best manner of bringing up game cocks, while young, is in a farm yard, in as free an air, and as much agreeable to nature as possible.

About three weeks or a month before they were to fight, I put them up, as it is called, or put them in a dark close penn, about two feet square. They are debilitated by being suffered to run among the penns, and their muscles are not firm. The first week I fed them upon barley, that is accounted a *scouring* food, but it answered best at the first period of their confinement. I fed them three times a day by measure, I cannot now ascertain the quantity, giving them very little water each time; and once a day, or once in two days, took them out to spar, or fight a few strokes with one another, with their spurs muffled. The second week, and during the most of the remainder of their confinement, I fed them on pure wheat, according to the same measure, having always regard to the state and regularity of their bowels, and giving a little barley, if they appeared costive. During the last three or four days I gave them white bread, according to the same measure, though I do not think bread was any better than wheat; and some that I fed entirely on wheat, after the first week seemed to do quite as well as those which had bread.

This was the whole of the process which I employed. I could always tell, by the firmness of the breast, whether my cocks were in order. I found them by far the strongest, without diminishing their activity, when they were *plump* but firm, without fat; and I question but they would have eaten as fine; and had nearly as much firm muscular flesh as a fowl from a London poulterer's. With this mode of management my cocks were four out of five, at least, successful.

3d. The training of the cocks, in the manner I have described, produces only a temporary effect; nor does it in the least seem to shorten their lives. I have known them live and fight at ten years old; whereas the poultry in my yard at present seldom reach that period.

4. I have heard of saffron and other drugs being given to cocks; but mine, which were plainly fed, always beat them. Opium or brandy may be necessary to Janisaries or Frenchmen,

men, but no dram is necessary to excite the courage of a true game cock, or a British soldier.

Information respecting breeding, training, and management of game cocks.

The Rev. Mr. P——— is a native of Yorkshire, and may possibly be able to give you some information on the breeding of horses, and the training of jockies. At all events, an application to him, mentioning my name, can do no harm, and you will find him an obliging and intelligent man. He lately sent me a letter on the culture of spring wheat, which I sent to the board.

I am, dear Sir,
With great respect, &c.

P. S. I had forgotten one fact worthy of notice; when a cock had been fought so hard that he is even *apparently dead*, I have known him restored to life by covering him up, all but his head, in a warm horse dunghill, or a common hot-bed in a garden. On this you may depend, and I have no doubt that the cocks I speak of would have died but for this treatment.

A short Account of the Manners in which Game Cocks are bred up and trained for fighting. By an experienced Feeder.

It is a general principle in breeding cocks, that large bones are not desirable, but that large muscles are. The thigh should be long, with as much muscle as possible. The legs should be of a medium length, and not short like the Bantam breed. They cannot stand too high if the thighs are long. They should be round bodied and not deep (cailled) breasted. A small head is of essential importance, and it is a good sign to be hazle-eyed with black eye-brows. The black breasted red cocks in general stand the penn better than any other sort.

Parentage is certainly of great consequence, though there is often a very material difference between cocks hatched at the same time and from the same parents. The blood principally comes from the female. The likeness or outward shape from the male. The hens of the game breed are very spirited and even violent, and will not suffer a strange cock to have any connection with them.

Breeding cocks *in and in*, or stale breed as it is called (that is keeping uniformly the same stock) is a very bad system. It reduces their size, and takes away their vigour to so great a degree, Information respecting breeding, training, and management of game cocks.

degree, that they can hardly propagate their species, and the same is remarked in horses. If game cocks are bred in and in, they will stand to be killed without flinching, but they have not spirit or activity enough to attack their foes with any effect. If intended for fighting, they should never be crossed with dunghill fowls, for any taint of that blood makes them unfit for a long contest. The best plan is, occasionally to cross with some of the game breed of a different stock.

It is of great importance to have cocks *inwardly clean*, that is free from fat, for on that depends their being in wind. Neither race horses nor game cocks that are inwardly fat can be in wind. To give them a good constitution, it is better to keep them as much as possible in the open air, on a grass-plot, and with a gravel walk to go to. The more gravelly the soil on which they are kept the better. Yards are dangerous, more especially where horses are physicked, as the cocks may pick up what may do them mischief. Cleanliness is particularly necessary. When young, the chickens are kept with the hen under a hutch, and fed with oat groats; when they become older they get unhulled barley, which is reckoned more nourishing than oats. When they are put up to fight they are kept in small penns and fed for three or four days with the very best barley. For drink they get about a gill and a half of water per day, of as soft a quality as possible, and with a little toasted bread put into it to make it still softer. During the remainder of their stay in the penns, they are fed on one third wheat and two thirds barley, which is a nourishing diet, without being too costive. They are fed twice a-day, early in the morning, and at eight at night. Before being fed the second time, the crop is examined to see that it is quite empty and the food digested. They ought not to have before they are put into the penns, above three or four hens with them, and none after.

About four or five days before fighting they are physicked. The best medicine is about half a table-spoonful of cream of tar[illegible] made up with butter into a pill. This they can easily be made to take. The object is only to give them only two or three loose stools, which lightens them, and makes their flesh afterwards firmer. The day they are physicked they get nothing but a little warm water. Next morning they are put again on their hard feed of one-third wheat and two-thirds barley, and in the evening of that day they get a *hot meal*, consisting

ing of wheat bread and milk, with a little white sugar candy. More than one meal of that sort would make them heavy or lumpy. In the summer season, after being physicked, they get air the second day, but in the winter they ought to be kept warm, without being at the same time too hot.

Information respecting breeding, training, and management of game cocks.

Brandy, or any heating drug on the day of fighting, does more harm than good. They may get, however, just before they set to, a few barley corns, with a little real sherry wine.

A cock's first battle is his best, and a cock first penned, of equal goodness, will beat a double penned one.

Game cocks live fully as long as common fowls. In some cases they have lasted above fourteen years, and as sound as the first day. They are so hardy that they can be reared in the winter time much better than the dunghill sort. The cross between a game cock and a dunghill hen is excellent eating either as chickens or fowls.

XII.

Observations on the Culture, Properties, and comparative Strength of Hemp, and other vegetable Fibres, the Growth of the East Indies. By Dr. WILLIAM ROXBURGH *.

(Continued from Page 47 of our XIth Volume.)

TO prove the durability of the various materials formerly mentioned, I had recourse to maceration in fresh water, during the hot season. The result of these trials will be found in the following table, which, in a great measure, corresponds with the former, showing the comparative strength of the various cords mentioned therein, by weights suspended by four feet lengths of them. The first three columns on the left, have been explained in the first part of these observations; in this the largest cord only of each sort has been inserted. The three last on the same side express the average weight at which each sort of cord broke, after having been kept at the bottom of muddy, half putrid, stagnant pond water, from the 27th of February to the 22d of June, 1801.

Course of experiments.

From

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* From the Memoirs of the Society of Arts, 1806.

Materials, or Names of the Plants which yielded them.	Average weight at which each sort of cord broke.					
	When fresh.			After 116 days maceration.		
	White.	Tanned.	Tarred.	White.	Tanned.	Tarred.
1 Hemp from England . . .	105	—	—	—	Rotten	—
2 Ditto growth of India . .	74	139	45	A	Rotten	—
3 Coir	87	—	—	—	—	—
4 Ejoo	96	—	—	94	—	—
5 Robinia cannabina, ripe.	88	101	84	40	56	65
6 The same cut while blossoming	46	61	48	Rotten	68	45
7 Crotalaria juncea	68	69	60	Rotten	51	65
8 Corchorus olitorius . . .	—	—	—	—	—	—
9 Corchorus capsularis . .	67	—	—	50	—	—
10 Flax, growth of India . .	39	—	—	Rotten	—	—
11 Agave Americana	110	79	78	Rotten	Rotten	15½
12 Aletris nervosus	120	73	48	30	26	34
13 Theobroma Augusta Linn.	74	58	44	38	54	50
14 Theobroma guazuma, Hort. Cliff	52	47	45	30	39	—
15 Hibiscus tiliaceus	41	62	61	40	55	70
16 Hibiscus Manihot	61	—	—	26	—	—
17 Hibiscus mutabilis	45	53	—	Rotten	45	—
18 Hibiscus, from Cape of Good Hope	22	—	—	17	—	—
19 Bauhina, a scandent species	69	—	—	Rotten	—	—
20 The same, but differently prepared . . .	56	—	—	Rotten	—	—
21 Sterculia villosa	53	—	—	30	—	—

East India fibrous vegetables rot less than European.

Stagnant fresh water, in a rather putrid state, during the hot months of March, April, May, and June, in Bengal, must be as severe a trial for vegetable fibres, as can be well found in any country. I am exceedingly glad to find that, in general, the fibres of our East India plants stood the test infinitely better than hemp from England, or of hemp or flax the growth of Bengal.

Tar preserves them.

Tar appears in general to be a better preservative than tan during the immersion, though I was formerly inclined to think otherwise. The powers of No. 4, to resist decay, correspond with what the Dutch historian Rumphius says of it in his Herbarium Amboynense. Nos. 7, 8, 9, and 13, 14, retained their strength surprisingly. No. 15, (the bark with which the inhabitants of the South Sea islands make lines), gained considerably in power in its tarred state.

Additional plants fit for cordage.

In the former part of these observations it was remarked, that numerous plants, exclusive of those which yield hemp and flax, were productive of fibres apparently well qualified for the same useful purposes; and these several sorts are pointed out, some of which had been long and well known to the natives of Asia: others appeared to me to be unknown to them. Since the date of that paper, my researches have brought to light several additional objects of the same nature, and added considerably to the imperfect knowledge I then had of others. At the close of my first experiments (vol. xxii. page 395-6) mention is made of the strength of sun cords being greatly increased while thoroughly wet with fresh water. From 100 to 200 additional experiments have been made since that time, to illustrate this interesting fact, the result of which will be found in the two last columns of the annexed table.

How fabricated.

The cords now employed were made of three single yarns; and, as formerly, by no means so equally spun, or laid, as might have been done by an expert European artist: nor must their strength be compared with those of the same material in the former table, because the cords are now made considerably stouter, and the yarns are, in general, better laid, on account of their being thicker; for I suspect that the smallness of the lines employed in the former trials rendered them somewhat less accurate than the present.

Comparative Statement of the Strength of the various Materials employed in these Experiments, both dry and wet, by Weights suspended by Four Feet Lengths of the Cords.

Names of the Plants, or Materials used, and brief Remarks thereon.	Average Weight by which each Cord broke when dry.	Average Weight by which each Cord broke when wet.	Average of Cents. by wetting the Cords.
1 Hemp, the growth of 1800, from the Company's hemp-farm, near Calcutta	158	190	20
2 Jeetee of the Rajemahl Mountaineers, the clean fibres of the bark of a new species of Asclepias	248	343	38
3 This cord is made of 15 threads of fine sail-twine from Bencoolen, the produce of a new shrubby species of nettle (Urtica)	240	278	16
4 Sun (Crotalaria juncea) cut before the plants were in blossom, and steeped immediately	112	158	41
5 The same, and cut at the same time; but the plant was dried, or rather kept for some days before it was steeped	60	78	30
6 The same, cut when in full blossom, and steeped immediately .	130	185	42
7 The same as No 6, and cut at the same time, but attempted to be cut before it was steeped	100	166	66
8 The same, cut when the seed was perfectly ripe, and steeped immediately	150	203	35
9 The same as No. 8, and cut at the same time, &c. as No. 5 and 7 .	110	163	48
10 Sun, the winter crop; the seed from the coast of Coromandel, cut when the seed was ripe, and steeped immediately	160	209	31
11 A variety of Corchorus capsularis, Teetah-paat of the Bengalese, delicately fine like flax	143	146	2
12 Reddish Corchorus capsularis; the seed imported from China; also fine and soft like flax	164	164	0

Names of the Plants, or Materials used, and brief Remarks thereon.	Average Weight by which each Cord broke when dry.	Average Weight by which each Cord broke when wet.	Average of Cents. by wetting the Cords.
13 Corchorus olitorius, Bunghi-paat of the Bengalese, and very like the two last	113	125	11
14 Robinia cannabina, Dansha of the Bengalese, cut when the seed was nearly ripe	138	145	5
15 Abroma Augusta, Hort. Kew. (No. 13 of my former paper) young shoots before the flowers opened .	100	112	12
16 The same, from ligneous plants which had ripened their seed . .	121	121	0
17 Hibiscus strictus (No. 16 of my former paper, and then mistaken for Hibiscus Manihot) cut when early in blossom	104	115	10
18 The same, but cut when the seeds were perfectly ripe	128	135	5
19 Hibiscus cannabinus, Meesta-paat of the Bengalese, cut when in flower, and steeped immediately .	115	133	15
20 The same, (Gong-kura of the Telingas) cut when the seed was ripe .	110	118	7
21 Hibiscus, from the Cape of Good Hope (No. 18 of the former paper)	116	123	6
22 Hibiscus sabdariffa, cut when in flower, and steeped immediately .	89	117	31
23 Hibiscus Abelmoschus, Kalee-Kustooree of the Hindoos, cut when in flower, and steeped immediately	107	107	0
24 Hibiscus esculentus d'Heroos of the Bengalese, cut when in seed, and steeped a few days after . . .	79	95	20
25 Hibiscus bifurcatus, cut when in flower, and steeped immediately	89	92	3
26 Hibiscus pilosus, an annual, cut when in advanced flower, and steeped immediately	97	130	34
27 Theobroma guazuma, young tender shoots from the roots of small trees steeped immediately	100	140	40
28 Fibres of the foot-stalks of a very large new species of plantain, (Musa superba)	78	0	0

Remarks on some of the Plants or Materials mentioned in the annexed Table.

Jeetee, a strong material, hitherto unknown to Europeans.

No. 2. Jeetee, of the Rajemahl-hill people, who make their bow-strings of it, which are said to last five years, though in constant use, and exposed to all sorts of weather. It is the fibre of the bark of a very extensive twining, shrubby, plant, a new species of Asclepias, discovered by my son, Mr. Wm. Roxburgh, in 1800, growing wild on the tops of the hills, in the vicinity of Rajemahl. The fibres are prepared by stripping off the bark from the tender, succulent shoots, during the rainy season, when they are full of sap, and by removing the pulpy parts with the nails, or with a piece of sharp-edged hard stick upon a board. Hitherto this beautiful, strong material, has been unknown to Europeans, and, so far as I have yet been able to learn, only employed by the people of those hilly or mountainous tracts, to make their bow-strings; consequently, it will be difficult to ascertain the quantity that may be annually procured, or the price. All I can say at present is, that four pounds weight of the clean fibres, a friend procured for me, for one rupee (half-a-crown). A drawing and description of this beautiful and useful plant, is in the possession of the Honourable the Court of Directors, under the name of Asclepias Tenacissima.

Poolay of the Malays.

No. 3. Calooee, or Battang-calooee, or Poolay of the Malays, a new, shrubby species of Urtica. The cord employed was made of fine sail-twine, sent from Bencoolen by Mr. Ewer, and was made by the Malays of that place. Its strength is very great, and the beauty and fineness of the fibre adds to its recommendation. But what quantity is procurable, or the price of it, I am at present unacquainted. The plant has, however, been introduced into the botanic-garden at Calcutta, where, in little more than one year, above one thousand plants were reared from four, which were received from Mr. Ewer, the Governor of Bencoolen. I made a drawing and description, now in the possession of the Honourable the Court of Directors, of one of these plants, which flowered in November, 1804.

The Sun plant.

No. 4 to 9. The sun, on which the experiments stated in the table were made, was sown between the 11th and 13th of June, 1801, and cut at fourteen different ages, beginning on the 26th of July following, before the blossoms appeared, and ending the 14th of September, when the seed was fully ripe. The fibres of these different crops were prepared in different ways; viz. by macerating for a longer or shorter time; by steeping immediately after being cut; by being half dried; or dried as well as the season would permit, as practised on the coast of Coromandel.

The average result of these various trials will be found reduced to six numbers, in the table. But it is necessary to observe, that the constant wet or very damp weather, which prevails in Bengal at this season, renders it almost impossible to dry the plant, and must injure the quality of the fibre: indeed few seasons will admit of drying the plant to any extent. Various experiments, from half a day to half a year's drying, and keeping, were made, with the view of ascertaining whether steeping immediately after the plant was pulled, or at any other period, was the best for retaining the full strength of the fibre; and I have reason to believe, that immediate steeping is to be preferred, at least in Bengal, during the rains.

No. 10. This was a few square yards of sun, reared from the seed, which was received from Ganjam, on the coast of Coromandel; and though sown with the rest of the seeds in June, did not blossom till the close of the rains in October, nor ripened its seed till January. This sort I would call Winter Sun, because what is generally cultivated in Bengal, requires only about three months from the time of sowing (middle of June) for the ripening of the seed (September). Another sort.

From the experiments made, I am led to draw the following conclusions, viz.

1st. That the fibres of this material are softer and finer when the seed is sown thick, and the plant cut as early as the flowering season, or rather before, and that they become coarser progressively, till the plant, which is annual, perishes. Conclusions or general remarks on the Sun-plant.

2d. That the fibres are at their greatest strength when the seed is ripe, which corresponds with the opinion of the natives. At this period the crop requires about one third more time to complete the maceration, than if cut at the flowering-season; it may amount to from 48 to 72 hours, according to the warmth of the water, and the state of the weather. Deep water requires more time to complete the operation than shallow water, which is generally some degrees warmer.

3d. That the sooner the plant is committed to the water, after being cut, the better: probably, because during the rains it is very difficult to dry it, and on that account the strength of the fibre will be weakened and the colour injured. Besides, in cleaning or dressing the Sun by the usual modes practised in Europe for hemp, I found, on an average, that the original quantity was reduced only one third, when the plant was steeped immediately, and nearly one half, if kept with the view of drying it before it is put into water.

4th. I found the practice of drying the plant, after maceration, and previously to the removal of the bark, as followed in Europe with hemp and flax, by no means advantageous, but prejudicial.

For an account of the plant Crotalaria Juncea, which produces the Sun, and for the method of cultivating it on the coast of Coromandel, which is different from that in Bengal, consult the Coromandel Plants, Vol. II. No. 193.

No. 11, 12, 13. As a substitute for flax, these seem to deserve attention, on account of the length, strength, and fineness of the fibre, and from the durability and strength of it, after 116 days maceration. The seed of No. 12 was brought from Canton in China, under the name of China Hemp, and grows as freely in Bengal as the sorts in general cultivation there. But while the produce was fully as great, the quality was better, which induced me to distribute the seed among the natives, and recommend the cultivation of this sort in preference to No 11 and 13.

Abroma Augusta.

No. 15 and 16. Abroma Augusta, of the Hortus Kewensis, called by the younger Linnæus Theobroma Augusta, in his Supplementum Plantarum; and Woollet Comal of the Bengalese. My remarks of the 31st of January, 1801, closed with an account of this plant. The Biggah there mentioned, or rather half Biggah (for by measurement it proved to be no more) yielded two luxuriant cuttings during the hot and rainy seasons; and a third of a more limited growth by the end of the cold season. For while the cool northerly wind prevails, during November, December and January, the plant grows but little. The quantity of clean fibre obtained from the two first cuttings, weighed 245lb. avoirdupois; and from the third, 26lb. making together 271lb. which is a produce three times greater than the average produce of Sun from the same quantity of land. As the plants, though nearly two years old, are still luxuriant, I have reason to expect that the average produce will be for many years to come as great, if not greater, than it was last year. Another great advantage in favour of the Abroma Augusta is, that the Sun requires to be drest, for convenience of stowage, prior to its being shipped, by which it loses about one third of its weight: the fibres of the Abroma Augusta are naturally clean and white, and do not, in my opinion, require dressing.

New species of hibiscus.

No. 17 and 18 is No. 16 of my former communication, and there called Hibiscus Maniho. It is, however, I am now convinced, an undescribed species from the Moluccas, which I call H. Strictus, on account of the remarkable straightness of the stem and branches. Last year, all the fibres of the few plants I then had, were only sufficient to make one line. The experiments on this beautiful material were, on that account, very limited. Nevertheless, the seed collected from these few plants produced plants sufficient to fill 40 square yards of land, and yielded 33 pounds weight of the naturally clean fibre from one cutting; and, as it is a short-lived annual, does not yield any second crop. A drawing and description of this plant is in the possession of the Honourable the Court of Directors.

Hibiscus cannabinus.

No. 19 and 20. Hibiscus Cannabinus, an annual universally known over India, and in many parts cultivated, not only for the fibres of its bark, but also for its green leaves, which are of an agreeably acid flavour, not unlike sorrel, and used by the Hindoos as a pot-herb. For a drawing and description of the plant, consult Coromandel Plants, Vol. II. No. 190.

Ejoo, a very valuable palm.

Ejoo, *No. 4, of my former memoir, in which it is observed, that this very valuable and beautiful tree is found to grow well in Bengal. Since that time I have attended particularly to its growth, and found that, on an average, each tree produces about six leaves in the year, and that each leaf yields 10½ ounces of ejoo (the black horse-hair-like fibres † employed for

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* Palma Indica venifera; and Rumph, Anab. vol. i, p. 57, t. 13. Arenga saccharifera of Labillardiere. Anon. Marsden's History of Sumatra, p. 77.

† The fibres grow from the base of the footstalks (stip.) of each leaf (frond.) and embrace completely the trunk of the tree. The fibres and leaves are easily removed without injuring the tree.

for making cordage) which makes the annual produce of each tree within a fraction of four pounds*.

Besides, this palm abounds, probably, more than any other in wine, furnishing sugar and ardent spirits; and when the tree arrives at maturity, the pith of it is one of the varieties of sago-meal, used by these people in their diet. Hence we have every reason to think, that it will prove one of the most profitable trees which can be cultivated in warm countries, at least in those where it will grow freely.

I had various other plants in cultivation for further experiments, when bad health obliged me to desist and come to England. From the following I had prepared the fibres, but had not made any experiments on their strength.

Agave Tuberosa.

Agave Tuberosa of the Hortus Kewensis. The large leaves of this elegant species, which has lately been introduced into Bengal, are replete with strong white fibres, far superior in appearance to those of Agave Americana (No. 11 of my first paper.)

Musæ. The plantain, in its wild state, abounds in strong fibres, more or less fine. The species which we call Coccinea, yields what is called Manilla hemp: at least it was sent to me from China as that plant.

Helicteris isora

Helicteris Isora. The inhabitants of the Malabar mountains employ this material for making twine and cordage. In Wynaad they call it Ky-walla-nara. It is strong, but rather coarse, and of a dull colour. Various species of Sida, particularly Rhombifolia, and Periplocifolia, yield uncommonly fine fibres. In Rhombifolia they are particularly delicate. Urenalobata and Sinuata also abound with them. In short, the whole of the plants of this extensive natural order, called by Linnæus, Columniferæ, and by Jussieu, Malvaceæ, are furnished with substitutes for hemp and flax.

P. S. Samples of most of the materials mentioned in this paper, I have in my possession; and it is my intention to deposit them in the East-India Company's Museum, in Leadenhall Street.

XIII.

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* Some of the very best trees I have found to produce fully one pound of the fibres in each leaf.

XIII.

Some Account of a very singular and important Alum Mine near Glasgow, at present worked by Messrs. MACKINTOSH, KNOX, *and Co. Taken by Dictation from Mr.* KNOX, *by the Editor.*

Wonderful excavation of a mile in extent.

AT Hurlett, near Glasgow, the works of Messrs. Mackintosh, Knox, and Co. the aluminous schistus lies 10 inches thick above a coal, at all distances from the day. It is at present worked at the depth of 30 fathoms over a coal-pit worked for three centuries, and now in work. The dip is just sufficient to keep the whole excavation quite dry; and the schistus above becomes decomposed by oxigenation, and falls down* in consequence of the working maintained during that long series, constantly in the same apartment and at the lowest point. The excavation is now at the prodigious dimensions of a mile in length, and little less in breadth. The coal stratum thus taken out, is very regularly 5 feet thick. About the year 1620, a tack or lease still extant, describes it as an extensive going work; and particular precaution is taken lest the tenant should work the whole of it out. It is upon the estate of the Earl of Glasgow; and the schistus is the same as is alluded to by Dr. Black in his lectures. The alum work is perhaps the largest single work in Great Britain, and probably in the whole world; and this article being now equal in quality to that of Italy, is exported to foreign parts instead of our manufactures being dependant on supplies from abroad as formerly.

—of great antiquity.

Curious and extensive oxigenation.

The whole roof of this immense cavity being exposed to the atmospheric air, is in a state of gradual decomposition. This process is so slow, that in the long period of time before mentioned, the full roof of 10 inches is in no place gone. It flakes off by the oxigenation, and falls down; in which last situation, the oxigenation goes on upon the dry floor, and swells up the mass of a fine light specular efflorescence to the height of three, four, and even the whole five feet of the excavation.

Singular combination of circumstances.

The combination of circumstances in this work, are very extraordinary. Had the schistus been disposed on the floor, instead of the roof, the oxiginated surface could not have fallen off; but would have covered the inferior portion, and put an eternal stop to the process. Had the coal work been carried on from the lower to the higher part, the waters would have been left to accumulate, and would have dissolved the efflorescence as fast as it was formed. Or, had this extensive simple apartment been abandoned as is usual in collieries, at much less periods, the same effect would have followed. And, with all these advantages, if the length of time had been less, or the extent of surface more limited, the slow process of efflorescence would have been totally inadequate to the supply of a manufactory. Or lastly, if a greater number than usual of pits had not been left unfilled, no circulation of air equivalent to the efflorescence could have followed.

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* In the coal excavation, one-fourth has been left as pillars to support it. They are round, and about 18 feet diameter.

Efflorescence resembling a field of corn.

In these pits is found a very singular efflorescence of sulphate of magnesia growing in fine spiculæ, about a foot in length, and covering a space of 40 or 50 yards square, like a crop of corn. It has been much injured by visitors who have trodden it down and taken parts away.

Lime stone.

Lime-stone is got at the same works. It lies over the schistus, generally about 3 feet thick, more or less. It is horizontally separated into two by a very thin seam of crystallization, in which the miners make their blast, which throws down the lower portion, and leaves the upper as the roof.

Pyrites.

Very beautiful effloresced pyrites, the residue of the coal works, are found among the decomposed schistus, and are worked with other pyrites for copperas.

Specimens of the schistus in its several states.

Schistus, No. 1.—Ten inches thick native material, very dense even with conchoidal fracture.

2.—First stage of decomposition; Dirty light brown exterially, with efflorescence, and numerous small cracks throughout, shewing the slaty texture.

3.—Third state: More split and weathered.—Many parts flaked off. White saline, dusty thin covering of efflorescence, and saline matter in the cracks evidently forcing the them asunder. The salt tastes rough, acid, and ferruginous. Slight wooly, or silky appearance here and there like the flowers of Benzion.

Fourth

4.—Fourth state: Light white, or very pale greenish white mass, consisting of the silky or fine fibrous salt, intermixed with flaky fragments of the yet undecomposed schistus. In two of the specimens where the damp has operated, the efflorescent salt lies closer, is more adherent to the schistus, and is greener in some places like sulphate of iron. The salt is very soluble in water, and half the weight of the mineral in the state No. 4, is taken up by that fluid.

XIV.

Method of weaving Cloth of a surprizingly fine Quality. By Mr. WILLIAM NEVEN.*

Very fine cloth made of 256 shoots in the inch.

THE inventor acquaints the secretary that he has discovered an improvement in the art of weaving, which certainly will turn out a great national advantage.

By this improvement cotton, linen, and silk goods, can be made much sooner and finer, than by any method yet discovered. Upon this principle he has made a small piece of plain silk cloth, from hard thrown silk in the gum, that contains the amazing quantity, of 65,536 meshes in one square inch, or 256 threads in the inch of the side, which is double the number in any cloth before made.

It is impossible for any reed-maker to make a reed half so fine as to weave such cloth upon the present principles of weaving; and even if that could be done, no weaver could make use of it: but by this method, he may weave, as fine cloth in a twelve hundred reed as by the present method in one of twenty-four hundred, and with rather less than more trouble.

He sent specimens of both silk and cotton cloth, woven upon this principle, and material advantage may be derived from this plan in making cambrics, muslins, &c.

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* Soc Arts, 1806.

The method is very simple. More threads of the warp than usual are passed between the dents of the reed.

The method as it was explained to a Committee of the Society, consists in adding more thread of the warp within each dent or split of the reed than in the common way; for instance, that where in the common mode there are only two threads in the reed, there are upon his plan three or four.

The weft or shoot is thrown in the common way with a single thread.

When the cloth is woven and taken out of the loom, it has the appearance of being barred or striped, the cane of the reed occasioning that part of the cloth struck with it to look thinner, owing to the threads of the warp being further apart.

It comes out quite fair when wetted and pulled.

The cloth is then to be wet in water, and in that state to be repeatedly stretched across by the hands backwards and forwards corner ways; by this means the threads, which apparently formed the stripe, or close part of the cloth, separate from each other, and become diffused at equal distances. The appearance of stripes being entirely removed, the cloth becomes of unexampled fineness, and extremely regular in its texture. This operation must, in cotton fabrics, be performed before the cloth goes to the bleach-ground.

Silk goods, on being taken out of the loom, must be wet and well rubbed, as in common mode of washing, and then stretched backwards and forwards, as in the manner above directed for cotten goods.

In silk goods, the warp and weft may be both alike; in cotton goods the weft may be softer, but of the same fineness.

Mr. Neven stated, that fine linen cambrics may be made much superior to any hitherto made in France; and that though there are three threads within each dent, or split of the reed, whilst the cloath is weaving, yet the headles or yields lift up the threads alternately throughout the whole breadth of the cloth, and that there are about 250 shoots in an inch.

XV.

Extract of a Letter from Mr. H. STEINHAUER, *dated Fulneck, Jan.* 30, 1807.

IF the phenomenon below described has already attracted notice, or if you think it unworthy of it, I beg you to consign it to oblivion; but if it is worth attention, it may perhaps find its way into some corner of your valuable Journal.

New kind of halo.

June 28, 1805, between 10 and 12 A.M. being off the coast of North Wales, three or four miles, and the ship going with a pleasant side-wind, about three knots per hour, I was agreeably surprised, while standing on deck, to perceive the shadow of my head in the water, environed by a luminous circle, apparently eight or nine feet in diameter, of a brilliant white colour. It appeared as if formed by the reflexion of the rays of the sun upon minute white particles in the water. However, neither sand nor perceptible globules of air could be discovered upon close examination. A similar appearance, surrounding the shadow of the head, upon the dewy grass, is occasionally observed shortly after sun-rise, but tinged with prismatic colours, and of small diameter. The circular rainbow in the spray of water-falls, bears a nearer resemblance to the above-mentioned appearance; but there was here no *perceptible* spray, and an *imperceptible* one could hardly produce the brilliant appearance. I merely mention the fact, as I do not remember to have seen it noticed, as it may perhaps serve to elucidate some hypothesis, or be applied as an example of some law, observed by an unprejudiced person.

I need hardly add, that though several viewed the phenomenon, each saw the luminous halo *only around his own head*.

XVI.

Letter to the Editor, concerning the Blacking for Leather.

SIR,

YOU will not, I trust, be offended at being consulted respecting a manufacture, humble indeed its kind, but of no small importance in domestic economy. I mean, blacking for shoes and boots. I have seen several recipes for liquid blacking, all of which appeared to be ill-combined farragos—all of them containing several ingredients either useless or hurtful. The bases of all were ivory black, oil, and vitriolic acid; but in such vague and indeterminate proportions, such as two-pennyworth, that there could be no certainty of a perfect saturation, nor any security of the leather from injury. Certain it is, my shoes and boots do not last so long as they were wont some twenty years ago; but whether this is owing to the corrosive quality of the new-fashioned pigment now in use, or to the more expeditious, although probably less perfect process of tanning, I am at a loss to determine. If you, Sir, or some of your ingenious correspondents will favour the writer and the public with a good recipe for liquid blacking, you will not only render a service to both, but likewise prove the means of abolishing the frequent but nefarious practice of extorting sums of money (one to five guineas) from credulous footmen and their sapient masters, for worthless or pernicious nostrums.

I remain, Sir,
Your constant reader,
and obedient humble Servant,

Bristol, 26 *Jan.* 1807. C.

Reply.

The disposition to exhibit marks of the most fastidious neatness in our clothing, is one of the characters of refined society. To give a glossy black surface to leather, when cleaned, is considered as one of the requisites for this intention; but I do not know that any of the pigments hitherto used are entitled to much commendation for the effects which the venders ascribe to them. To render leather flexible, soft, and impenetrable to water, and at the same time shining, does not seem to be practicable. Oil-grease and bees-wax, with lamp-black, or ivory-black, are the principal ingredients in the compositions for the former purpose; and the latter has usually been effected by sugar dissolved in beer or water, with the addition of the black. There does not appear to be any reason why the proportions should be considered as of any great consequence. For boots, or shoes, intended to keep out water, a compound of wax and tallow, with lamp-black, will probably be found amongst the most useful, if laid on before the fire, in order that the pores of the leather may be closed; but the aqueous compound will be preferrable, where the mere appearance is regarded.

SCIENTIFIC NEWS.

Astronomy.

Seeberg Observatory.

THE friends of astronomy will learn with pleasure, that the observatory of Seeberg, founded by the late duke of Saxe-Gotha, will not much longer remain in the deserted state it has languished in since the death of that prince. M. de Zach, who directed it, and had retired under the protection of the Dowager Duchess, will attend that princess into Italy. Consequently, he will resign his place of astronomer, which, it is hoped, will be given to the celebrated Dr. Olbers. The observations of Seeberg, after a short interval, will be renewed with new zeal, and will continue to add to the treasures of astronomy

Beavers in Westphalia.

Beavers in Germany.

A German Journal informs us, that beavers exist in Westphalia, on the banks of the Lippe, where they maintain their situation, notwithstanding the efforts of the inhabitants to destroy them. It is easy, say the narrators, to prove their existence, by the great number of felled trees on the banks of the river. Ought we to conclude from this remark that the beavers of this district live in society? The fact is of sufficient consequence to have deserved a more ample detail.

Letter from the Rev. PETER ROBERTS, *A. M.*

Wrexham, Denbighshire, Dec. 27, 1806.

SIR,

In looking back over the numbers of your interesting Magazine, I find, at the end of that for January last, a notice given of "An invention laid before the Celtic Academy, of a mode "of corresponding with men, whose language is unknown, "with expedition, without previous study, any expence, "the least trouble, or the smallest labour of the mind."

The little tract which accompanies this note, and which, as will appear by the date, was published in A. D. 1802, presents a mode of such correspondence, with the facilities above mentioned, in as great a degree as the subject is capable of; and as I presume you will think it just that whatever be attributed to such an invention, or the priority of invention, should be attributed where it is of right due, I take the liberty of requesting you will have the goodness to notice this in your Journal, in such manner as you shall judge to be the most proper.

Whether the mode presented to the Celtic Academy bears any resemblance to mine or not, I have not been able to learn, and can therefore make no comparison as to their relative advantages; but I hope you will allow me, Sir, to refer mine to your consideration, being persuaded that one so scientific will perceive the utility of my mode, and the ease with which it is practicable, as several others have done.

Should it appear so to your judgment, your noticing it as such will be a particular favour to,

Sir,
Your very obedient and humble Servant,
PETER ROBERTS.

The pamphlet with which the author has favoured me, is entitled " Art of Universal Correspondence," and is comprised in sixteen pages. The author's instructions are, that the correspondents be provided with a double dictionary of the two languages, and he uses marks or characters to denote the inflexions, the pronouns, and some of the most necessary of the conjugations. These characters being few in number, and simple as well as universal, are easily remembered and applied, and all the irregularities of that part of languages, which they represent are done away. The marks denote, 1st, the articles *a* and *the*, seldom used; 2d, gender, number, and case of substantives; 3d, comparison of adjectives; 4th, pronouns; 5th, tenses of verbs; and 6th, conjunctions. The ready application of these is shown by appropriate examples; in which, besides the general advantages of the system, the reduction is between one fourth and one third of the common extent of writing.

Printed by P. Da Ponte, 15, Poland-Street, Oxford-Street.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY

AND

THE ARTS.

APRIL, 1807.

ARTICLE I.

On the Inflammable Gas formed during the Distillation of Peat. By THOMAS THOMSON, *M. D. F. R. S. E. Communicated by the Author.*

Inflammable Gas by heat from vegetables.

IT is well known, that when vegetable substances are exposed to heat, in close vessels, they are decomposed, and yield, among other products, a considerable portion of inflammable air, which varies in the colour of its flame, in its specific gravity, and in its other properties, according to the substance from which it has been procured, or the degree of heat at which it has been evolved.

Their nature yet but little known.

The examination of these inflammable gases forms, at present, one of the most difficult branches of pneumatic chemistry. Neither their number nor constituents have been hitherto ascertained with precision; and some of the most sagacious and best-informed chemical philosophers have embraced opposite opinions respecting both. According to some, they may be all reduced to three gases, with which we are already sufficiently acquainted; while others consider them as liable to an infinity of variations, or limited only by the processes of the operator, and the number of substances from which they are obtained. That we are already acquainted with all the inflammable gases from vegetables, which it is possible to form, is an opinion which but ill accords with the present imperfect state of chemical knowledge. But it is to be hoped, for the future progress of the science, that the opposite doctrine is equally unfounded; for, were the number of such gases indefinite, the examination of them would be not only a disgusting and hopeless task, but altogether useless and nugatory.

Three at present distinguished

Three distinct inflammable gases from vegetables, are at present known, and characterized with considerable precision. These are,

I. Carbonic oxide

I. *Carbonic Oxide*, first accurately examined and analyzed by Mr Cruikshanks, and lately shewn by Mr William Henry, to be not unfrequently produced during the distillation of vegetable substances. It is characterized by a specific gravity, nearly equal to that of common air, by the blue flame with which it burns, the small quantity of oxygen which it consumes, and the great proportion of carbonic acid which it forms.

II. Carb. hydrog.

II. *Carbureted Hydrogen*, a gas which rises spontaneously from marshes in hot weather. Its specific gravity is exactly 6-10ths of that of common air: it burns with a white flame, consumes twice its bulk of oxygen gas, and forms exactly its own bulk of carbonic acid.

III. Olefiant

III. *Olefiant Gas, or Supercarbureted Hydrogen*, a gas which is procured by distilling a mixture of four parts sulphuric acid and one part alcohol, and which Mr. William Henry has shown to be evolved in great quantities during the combustion of pit-coal. It is characterized by the property which it has of losing its gaseous form, and assuming that of an oil, when mixed with three times its bulk of oxymuriatic acid gas. Its specific gravity is 9-10ths of that of common air. It burns with a yellowish white flame, like oils, and emits more light than any other gas. It consumes three times its bulk of oxygen gas, and forms twice its bulk of carbonic acid.

Opinion of chemists as to their nature.

The first of these gases is considered at present, by the greater number of chemists, as a compound of carbon and oxygen, the two last as compound of carbon and hydrogen, differing from each other in the proportion of carbon; the first containing the smaller, the second, the greater proportion of that constituent.

New gas: oxy-carburetted hydrogen.

If any confidence is to be put in the experiments immediately to be detailed, the gas which forms the subject of this paper constitutes a fourth species of inflammable air from vegetables, to which the name of *oxycarbureted* hydrogen may be given, a name already applied by Berthollet to carbonic oxide, but to which the gas from peat is much better entitled. If I shall be so unfortunate as to fall into error, I hope the difficulty of the subject will, in some measure, constitute an apology.

Gas from peat, examined by Henry.

Hitherto the gas obtained during the distillation of peat, has been examined only, as far as I know, by Mr. William Henry, of Manchester. But as the properties of his gas differ essentially from those of mine, it is very clear that the gases must have been of a different nature. Indeed, I have ascertained, by experiment, that different kinds of peat yield different kinds of gas, though I have not been so fortunate as to form any gas possessed of the properties which he describes; doubtless, because the peat which he employed was very different from any which I could procure.

The gas obtained by the Author was different. Characteristic of the peat.

The peat used in all my experiments was the kind commonly sold in Edinburgh: its quality was very indifferent; for it was soft, brown-coloured, and very spongy, and loose in its texture. Its specific gravity was only 0.600. When kept at the tenperature of 300°, it lost 1-4th of its weight. Between 400° and 500°, it smoked, and was charred, emitting the usual vapour of burning peat. When heated to redness, in close vessels, it left a very brittle charcoal, amounting to 1-4th of its weight. When burnt in the open air, it left a quantity of yellowish grey ashes, containing iron, amounting to 1-100th part of the peat. Good peat is much denser, not so easily decomposed, and approaches more closely to coal.

Experiments and observations on the inflammable gas from peat.

With this peat cut into small fragments, I sometimes filled an unglazed earthen retort, sometimes a cast-iron bottle, and then exposed these vessels respectively to a degree of heat which was purposely varied in the different processes. Sometimes the peat was kept for a considerable time at a temperature not exceeding 500°; and when all gas had ceased to come over, it was raised to a red heat. Sometimes it was placed at once in a strong red heat, and sometimes it was never allowed

Experiments and observations on the inflammable gas from peat.

to become red during the whole process. These variations were intended to ascertain how far the nature of the gas depended upon the temperature. But the results were not quite satisfactory. Sometimes the gas was the same, though the heat differed; and sometimes the gas varied, though all the circumstances of the process were as exactly as possible the same. The differences I am disposed to ascribe to variations in the properties of the peat employed. The gas began to come over very speedily. At first it was mixed with much carbonic acid; but the proportion of this gas diminished as the process advanced, though in one instance only it disappeared completely. The quantity of gas obtained from a given bulk of peat was much smaller than what is yielded by the same bulk of wood or pit-coal, owing probably to the great difference of weight between them.

I never succeeded in procuring the gas perfectly pure, as, besides the carbonic acid already mentioned, it always contained a portion of common air, varying from 1-8th to 1-4th of the mixture, according to the process. It was always greatest when the cast-iron bottles were used, and least with the stone-ware retorts; owing partly to the smaller size of the former, which did not allow me to throw away so great a proportion of the gas which first came over. The presence of common air cannot well be accounted for on any other supposition, than that the vessels were not altogether air-tight; for the tubes which conveyed the gas to the water-trough were very well filled. The stone-ware retorts are known already not to be impervious to air.

To remove the carbonic acid, I at first washed the gas in a large quantity of water; but finding afterwards that a portion of carbonic acid still remained, notwithstanding this process, I removed it, by washing the gas in lime-water.

To ascertain the proportion of common air contained in the gas, I employed nitrous gas, according to the method of Mr. Dalton, after having convinced myself of the accuracy of that method by repeated experiments. Into a long narrow tube, graduated to 100ths of a cubic inch, a portion of the gas to be examined, is introduced, and its bulk being noted exactly, a determinate quantity of nitrous gas, previously measured in a similar tube, is let up to it. If any common air be present, the bulk of the two gases gradually diminishes. The diminution of bulk, whatever it may be, is noted down, and multiplied by 0.36842: the product is equal to the measures of oxygen present in the inflammable gas. This quantity being multiplied by 5, gives the bulk of common air mixed with the gas very nearly.

Experiments and observations on the inflammable gas from peat.

By this process, I ascertained that the gas procured by the first distillation of peat, upon which the greater number of experiments were made, was a mixture of

Inflammable gas	88
Common air	12
	100

or it contained 12 per cent. of common air.

1. This gas had a peculiar empyreumatic smell, similar to that obtained from pit-coal and from vegetable substances in general by distillation. It was not deprived of this smell by agitation in pure water or lime-water. But after washing the gas in liquid oxymuriatic acid, I could no longer perceive it. This smell is usually ascribed by chemists to a small quantity of empyreumatic oil held in solution by the gas; an opinion not yet verified by any direct experiment.

2. It is not sensibly diminished by standing over water: oxymuriatic acid gas does not immediately produce any change on its bulk; a proof that it contains no sensible quantity of olefiant gas.

3. It is extremely deleterious to animals when drawn into the lungs. Some years ago, wanting to empty a large air-holder filled with gas evolved during the distillation of wood (which is probably similar to the gas from peat), I inadvertently applied my mouth to the pipe, to draw out the gas with more rapidity. The consequence was, that after about two inspirations, I dropt down on the floor insensible, and my servant, who supposed me dead, ran out in a fright for assistance, and had returned again before I recovered. On coming to myself, I recollected applying my mouth to the stop-cock, but was conscious of no uneasy sensation whatever previous to fainting. The recovery, however, was attended with very unpleasant sensations, which continued in some measure during the rest of the day.

4. Its specific gravity was 0.8358, that of common air being reckoned 1.000. To see whether the gas altered its nature

Experiments and observations on the inflammable gas from peat.

by keeping, it was left a month standing over an open trough of water. Its specific gravity was now found to be 0.8354, or about 1-2000th less than when newly deprived of its carbonic acid. Though these experiments were made with as much care as possible, I think it not unlikely that at least a part of this small difference may be owing to errors committed in weighing the air.

As the gas was not pure, but contained 12 per cent. of common air, it is obvious that it would have been lighter, if the air had been altogether absent. It is now perfectly established, that two gases, when mixed, do not sensibly change their bulk, unless they have the property of combining intimately, and of forming a new gas, which is not the case with the gas from peat and common air. We may therefore, from the preceding experiment, deduce the specific gravity of absolutely pure inflammable gas, from peat, by calculation.* This method gives us the specific gravity of the pure gas, 0.8128. Hence, 100 cubic inches of it, at the temperature of 60°, would weigh 25.18 grs. under a mean barometer.

When this gas is made to issue from a narrow aperture into the open air, and a lighted taper brought in contact with it, it catches fire, and burns with a beautiful bluish red flame. When mixed with common air in any proportion whatever that will burn, and kindled in a close vessel by an electric spark, the flame is always pale blue. If it be mixed with a small proportion of oxygen, it burns with a reddish blue flame; but with its own bulk of that gas, the flame is a fine white. After the combustion, a portion of carbonic acid may always be detected in the detonating vessel. The bulk of the mixture is always diminished after combustion.

6. To form precise notions of the changes produced upon this gas, by burning it with common air and with oxygen, a considerable number of experiments were essential; for as these experiments are necessarily made upon very small quantities of gas, we can hope for correct results only by taking the mean

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* Let A be the bulk of common air in the mixture; a, its specific gravity; B, the bulk of inflammable gas; x, its specific gravity, and c the specific gravity of the mixture. We have $A c + B c = A a + B x$; and $\frac{A c + B c - A a}{B} = x$. In the present case, $A = 12$, $a = 1\cdot$; $B = 88$, and $c = 0\cdot8354$; from which we deduce $x = 0.8128$.

Experiments and observations on the inflammable gas from peat.

mean of a great number of trials. The following was the plan which I followed.

(1) Before beginning each set of experiments, the inflammable gas was carefully examined by the method formerly described.

(2) Common air was always taken as composed of 21 parts of oxygen and 79 of azote.

(3) The purity of the oxygen gas employed, was ascertained in this manner: 20 measures of it were let up into a graduated tube, and 36 measures of nitrous gas added to them. The diminution of bulk was noted down, and multiplied by 0.36842. The product was the portion of oxygen contained in the 20 measures of oxygen gas employed. If this product amounted to 20, I concluded that my gas was absolutely pure; if it amounted to 10, I concluded that the gas contained half its bulk of azote, and so on; whatever was wanting to make up the 20 being always considered as azote. The oxygen gas employed was partly procured from manganese and partly from hyperoxymuriate of pot-ash. It was purposely employed of very different degrees of purity, in different experiments, as I wanted to ascertain the effect produced by the presence of different proportions of azote during the combustion of the gas.

(4) Thirty measures of the inflammable gas were generally employed. They were equal to 0.3 of a cubic inch. Each measure of oxygen and air was always equal to 1-100th part of a cubic inch.

(5) The gas and the oxygen were measured separately in narrow tubes, and then let up successively into a cylindrical glass tube, furnished with the requisite apparatus for passing an electric spark through it. This glass tube was previously filled either with water or mercury, according as the combustion was wanted to be over water or mercury.

(6) Immediately after the combustion, the residual gas was let up into a long narrow tube, to ascertain its bulk.

(7) It was then washed in lime-water, and the diminution of its bulk noted and abscribed to the absorption of carbonic acid gas.

(8) In some cases, the residue, thus freed from carbonic acid, was mixed with a fresh portion of oxygen gas, returned to the detonating tube, detonated a second time, the residue

Experiments and observations on the inflammable gas from peat.

measured, and then washed in lime-water, and measured again. But in the greater number of experiments, this repetition was not necessary, and therefore omitted.

(9) The residue, deprived of its carbonic acid, being put into a long narrow tube, a determinate quantity of nitrous gas was let up to it, and the diminution of bulk, if any, was noted and multiplied by 0.36842. The product was considered as equal to the measures of oxygen contained in the residual gas.

(10) This portion of oxygen, together with the azote known to have been present from the first, was subtracted from the residual gas, and the remainder, if any, considered as inflammable gas unconsumed.

(11) In some of the experiments, this method was considered as not sufficiently precise, and another was employed. The residual gas, deprived of its oxygen by nitrous gas, was washed in a saturated solution of sulphate of iron, till the whole nitrous gas was absorbed. From the remainder, the portion of azote present in the nitrous gas employed, was deducted. The residue was compared with the bulk of azote known to have been originally present. If it exceeded that quantity, the excess was considered as inflammable gas unconsumed.

From this account it will be perceived that there is some uncertainty respecting the residual unconsumed inflammable gas. We have no test to apply which can immediately indicate its presence; for it will not burn with oxygen, unless its quantity be considerable. Notwithstanding this uncertainty, by varying the proportion of oxygen, and its purity, we obtain results sufficiently satisfactory.

7. When the gas from peat is mixed with its own bulk of common air, it will not burn at all. But with two, three, four, and five times its own bulk of air, it burns. The combustion is most complete with three atmospheres. With five, the flame is extremely feeble, though most of the gas is consumed. The following table exhibits the result of my experiments. The gas used contained 12 per cent. of common air, and the experiments were made over water.

Experiments and observations on the inflammable gas from peat.

	Measures of Inflammable Gas.	Measures of Common Air.	Residue after Combustion.	Ditto, when washed in Lime water.	Nitrous Gas added to Residue.	Bulk of Mixture
1	30	30	No Combustion.			
2	21	42	54	51	12	60
3	30	90	92.5	84		
4	12	60	61	59	17	55

But before we can form any estimate of the result of these experiments, it will be necessary to separate the pure inflammable gas and oxygen from the azote, and likewise to note the composition of the residual gas, as indicated by the trials. This is done in the following table.

	Measures of Pure Gas.	Measures of Oxygen.	Measures of Azote.	Total.	Residue.	Carbonic acid formed.	Residue deprived of Carbonic acid.	Compos. of Residue		
								Oxygen.	Azote.	Gas.
1	26.4	6.8	26.8	60	No Combustion.					
2	18.5	9.32	35.18	63	54	3	51	1.10	35.18	14.72
3	26.4	19.4	74.2	120	92.5	8.5	84	6.00	74.2	3.8
4	10.6	12.8	48.60	72	61	2	59	7.72	48.6	2.68

From this table we obtain the following.

	Inflammable Gas consumed.	Oxygen consumed.	Diminution of bulk after carbonic acid is removed.	Carbonic acid forms.	
2	3.78	8.22	12	3	
3	22.6	13.4	36	8.5	
4	7.92	5.08	13	2	
	11.43	8.9	20.33	4.5	Average.
	100	77.86	177.86	39.37	Ditto per Cent.

Experiments and observations on the inflammable gas from peat.

From this table, it appears that the different experiments by no means agree with each other. In the second, for example, the oxygen which disappeared was more than double of the gas consumed, while, in the other two it was considerably less than the bulk of inflammable gas which disappeared. These anomalies, which appear at first sight irreconcileable, are in some measure accounted for by the subsequent experiments. In the mean time they shew us that common air cannot be employed in the analysis of the inflammable gas from peat.

8. If we substitute oxygen gas for common air, the combustion is more violent, and the detonation louder. If the proportion of oxygen be small compared with that of the inflammable gas, scarcely any of it can be detected in the residuary gas, after the detonation is over, though only a comparatively small portion of the inflammable gas is consumed. Hence, if we add a new portion of oxygen to the residuary gas, the mixture will detonate a second time, as loudly as at first. This double combustion continues till the oxygen amounts to about two thirds of the inflammable gas. If it be increased beyond that proportion, the residuary gas becomes incapable of burning, with what portion soever of new oxygen it may be mixed.

9. The first set of experiments were made with oxygen obtained from the black oxide of manganese. It was very impure, containing very nearly half its weight of azote. Two different quantities of this oxygen were employed. The first was composed of

57.9 oxygen
42.1 azote

100

The second portion was composed of

47.89 oxygen
52.11 azote

100

To those experiments, in which the first portion of oxygen was employed, the letter *a* is prefixed; while those in which the second portion was employed are distinguished by the letter *b*. The inflammable gas was the same as that employed in the preceding trials, and contained 12 per cent. of common air. The experiments were made over water. The following table exhibits the result of the experiments.

Experiments and observations on the inflammable gas from peat.

			FIRST COMBUSTION			SECOND COMBUSTION.					
		Measures of Gas used.	Measures of Oxygen.	Residue after Combustion.	Ditto, when washed in Lime water.	Measures of Oxygen added to Residuary Gas.	Residue after the 2d Combustion.	Ditto, when washed in Lime water.	Measures of Nitrous Gas added to Residue.	Bulk of Residue.	
a	1	30	8	No Combustion							
a	2	30	12	35	32	12	32	26	20	43	
a	3	30	16	33	28	16	34	28.5	21	38	
a	4	30	20	37.5	32	20	36	27	31	46	
a	5	30	24	37	30	24	42	32	29	43	
a	6	30	28	36	26.5	28	46	38	36	46	
a	7	30	32	39	27	32	59	59	44	47	No 2d Combus.
a	8	30	36	41	31				26	45	
b	9	20	40	38	28				27	34	
b	10	20	60	57	43.5				31	46	
b	11	20	80	78	61				48	46	
b	12	20	80	77	62				56	65	
b	13	20	80	77	64				49	58	
b	14	20	100	97	83				62	64	

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But before we can form any correct idea of the result of these experiments, it will be necessary to state the exact quantities of pure inflammable gas, of pure oxygen and azote present in each mixture, and likewise to note the composition of the residuary gas, as indicated by the analysis. All this is done in the succeeding table.

		FIRST COMBUSTION.				SECOND COMBUSTION						
						Addition.				Residue.		
	Measures of pure Gas.	Measures of pure Oxygen.	Measures of Azote.	Residue after Combustion.	Carbonic acid formed.	Oxygen.	Azote.	Residue after 2d. Combustion.	Carbonic acid formed.	Oxygen.	Azote.	Inflammable Gas.
2	26.4	7.71	7.89	35	3	6.96	5.04	32	6	1.10	12.93	11.97
3	26.4	10.03	9.57	33	5	9.28	6.72	34	5.5	4.23	16.29	7.98
4	26.4	12.35	11.25	37.5	5.5	11.60	8.40	36	9	4.42	19.65	2.93
5	26.4	14.67	12.93	37	7	13.92	10.08	42	10	6.63	23.01	2.36
6	26.4	16.99	14.61	36	9.5	16.24	11.76	46	8	10.31	26.47	1.22
7	26.4	19.31	16.29	39	12					2.10	16.29	8.61
8	26.4	21.63	17.97	41	10					5.16	17.97	7.87
9	17.6	19.70	22.70	38	10					4.05	22.70	1.25
10	17.6	29.30	33.10	57	13.5					10.50	-0.10	00
11	17.6	36.90	43.50	78	17					23.21	-3.7	0
12	17.6	38.90	43.50	77	15					19.52	-1.02	0
13	17.6	38.90	43.50	77	13					20.26	43.5	0.24
14	17.6	48.39	54.01	97	14					30.94	-1.95	0

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These experiments are of two kinds, each of which ought to be considered separately. In the first five the oxygen was applied in small doses, and the gas underwent two successive combustions. In the last eight, the proportion of oxygen was greater, and one combustion only took place.

By inspecting the first five experiments, it will appear that the inflammable gas was never entirely burned, but the residue diminished continually as the proportion of oxygen increased, and in the last of the list of them, did not exceed 1-22d part of the whole. If we examine the residual gas after the first combustion, scarcely any oxygen will be found in it: indeed, I could detect none at all, except when the proportion of oxygen approached that which limited the combustion to a single detonation. By subtracting the residual gas and the residual oxygen, after the second combustion, from the original quantities present, and by supposing the whole oxygen to disappear in the first combustion, we obtain the following table of the relative quantities of gas and oxygen consumed, and of carbonic acid formed, in these different experiments.

	FIRST COMBUSTION.			SECOND COMBUSTION.			
	Gas consumed.	Oxygen consumed.	Carbonic acid Gas formed.	Gas consumed.	Oxygen consumed	Carbonic acid Gas formed.	
2	2.29	7.71	3	12.14	5.86	6	
3	7.97	10.03	5	10.45	5.05	5.5	
4	5.65	12.35	5.5	7.32	7.18	9	
5	9.33	14.67	7	14.71	7.29	10	
6	14.51	16.99	9.5	10.67	5.93	8	
	7.95	12.35	6	11.16	6.26	7.7	Average.
	100	155.35	75.48	100	55.19	69	Average per cent.

From this table, it appears that the proportion of oxygen which disappeared by the first detonation, was much greater, compared with the inflammable gas consumed, than in the second combustion. The average of the first combustions gives

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155 measures of oxygen to 100 of the gas, while the average of the second gives us only 55 measures of the oxygen to 100 of the gas. In the first, the oxygen consumed was one half greater, and in the second, one half less, than the inflammable gas. The first detonation was always louder than the second, and accompanied by a white flame, while, in the second detonation, the gas always burns with a blue flame. The diminutions of bulk are always greater after the second detonation than after the first.

If we examine the individual experiments, we shall find that the proportion of oxygen consumed by the first detonation, is a maximum, when the smallest quantity of oxygen present is the smallest possible, and that it gradually diminishes as we increase the dose of oxygen. Thus, in the first experiment, of all, the oxygen consumed by the first combustion was to the gas consumed as 338 : 100; whereas, in the last experiment, it was only as 117 : 100. In the second combustion, on the contrary, the proportion of oxygen consumed rather increases with the dose. In the first experiment of all, it is not quite equal to half the gas, while, in the last, it is rather more than half the inflammable gas consumed.

If we consider all these circumstances, it will appear extremely probable that the effect of the first combustion is twofold: that one portion of the gas is burnt, while another combines with oxygen without undergoing combustion, and forms either carbonic acid, or some other inflammable gas still unknown. The portion of this new gas formed, diminishes with the doses of oxygen, because the proportion of gas completely burnt increases. It was doubtless the formation of this new gas, in variable proportions, according to the dose of air employed, that occasioned the variations in the result when the experiments were made with common air.

As the whole quantity of inflammable gas was never consumed in any one of the experiments in which the double detonation was employed, and as the residual gas most probably consists, at least in part, of the new inflammable gas formed during the experiments, it is obvious that we cannot depend upon these trials for determining correctly the proportion of oxygen which the gas from peat consumes. The average of the whole of them gives us 105.22 measures of oxygen as the proportion consumed by 100 measures of the gas. But, for the reason assigned, we must consider this quantity as rather excessive.

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As to the carbonic acid gas formed, we cannot draw any inference from the quantities obtained in these experiments, because they were made over water; for that liquid always absorbs a portion of this gas. The portion absorbed is variable, though in general it bears some relation to the violence of the detonation and the diminution of bulk produced by it—being always greatest when the diminution of bulk is greatest. But the real quantity of carbonic acid gas formed, can only be ascertained by repeating the experiments over mercury. This, in the present case, was not done, because I considered all the experiments with the double detonation as incapable of determining the objects which I wanted to ascertain.

From the eight experiments in which such proportions of oxygen were employed, as consumed the greatest part of the gas, by a single combustion, we deduce the following table.

	Measures of Gas consumed.	Measures of Oxygen consumed.	Carbonic acid Gas formed.	Diminution of bulk suppposing the Carbonic acid removed.	
7	17.79	17 21	12	35	
8	18.53	16.47	10	35	
9	16.35	15 65	10	32	
10	17.60	18.80	13.5	36.5	
11	17.60	15.69	17	39	
12	17.60	19.38	15	38	
13	17.36	18.54	13	36	
14	17 50	17 45	14	37	
	17.55	17.41	13.08		Average.
	100	99.20	74·53		Average per cent.

In

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In these experiments it deserves attention, that, after the proportion of oxygen employed exceeded a little that of the inflammable gas, there remained only a small portion of residual gas after the detonation, and that the whole of the inflammable gas was consumed when the oxygen was to the gas as 5 : 3, or in still greater proportions.

It deserves particular attention, that, in four of these experiments, the diminution of bulk is somewhat greater than can be accounted for by the quantity of inflammable gas and oxygen consumed. This small difference I ascribed, at first, to errors which had been committed in making the experiments. But after repeating each of them over again three or four times, with every possible precaution, the difference still continued as at first. I am disposed, therefore, to ascribe it to a small portion of the azote which was present, having combined with oxygen, and having formed nitric acid. We know that this happens when hydrogen, diluted with azote, is burnt with an excess of oxygen. The quantity is extremely small, and cannot materially affect the results: the only exception is the eleventh experiment, which does not correspond very well with the rest. The average of all these experiments gives us nearly 100 measures of oxygen gas consumed by 100 measures of inflammable gas, a proportion which cannot deviate far from the truth. The proportion of carbonic acid formed by the combustion of 100 measures of gas, is only 74.5 measures. But as the experiments were made over water, this proportion is rather too small. On repeating some of them over mercury, I obtained 80.5 measures of carbonic acid gas from 100 measures of inflammable gas consumed. These experiments then gave us the following proportions.

Gas consumed.	Oxygen consumed.	Diminution of bulk.	Carbonic acid formed
100	100	120	80

10. As the small portions of azote which disappeared in these experiments occasioned some ambiguity, I prepared some pure gas from the hyperoxymuriate of pot-ash. It was composed of

95.5 oxygen
4.5 azote

100.0

Having

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Having exhausted my whole stock of gas from peat, I prepared an additional quantity, which, after being freed from carbonic acid, was composed of

77 inflammable gas
23 common air

100

Its specific gravity was 0.8516, which gives us, for the specific gravity of the pure inflammable part, 0.8072. This gas, of course, is a little lighter than that used in the preceding experiments. But the difference does not amount to 4 per cent. one hundred cubic inches of it, at 60°, weigh 25.02 grains. The following table exhibits a view of the experiments made with this gas and the pure oxygen.

	Measures of Gas.	Measures of Oxygen.	Residue after Combustion.	Ditto, washed in Lime-water.	Nitrous gas added to Residue.	Bulk of Residue.
1	20	20	24	20	36	27.5
2	20	30	37	30	41	18
3	20	40	46.5	38	54	22
4	20	50	55	47	74	23
5	20	60	65	58	104	32

To understand these experiments, we must, as in the former case, separate the pure gas and oxygen from the azote, and state the nature of the residual gas, as ascertained by the analysis. This is done in the following table.

	Measures of Pure Gas.	Measures of Oxygen	Measures of Azote.	Residual Gas.	Carbonic Acid.	Residual Gas.		
						Oxygen	Azote.	Gas.
1	15.4	90	4.58	24	4	10.5	4.58	3.92
2	14.4	29.57	5.03	37	7	19.5	5.03	[illegible].17
3	15.4	39.12	5.48	46.5	8.5	25.8	5.48	6.72
4	15.4	47.67	5.93	55	8	36.1	5.93	4.97
5	15.4	58.22	6.38	65	7	47.9	6.38	3.72

It

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It is remarkable, that the whole of the gas was never consumed in any of these experiments, though there was present in every case a much greater proportion of oxygen than was necessary. Neither did the proportion of residue vary nearly as much as in the former case. The following table gives the proportion of gas and oxygen consumed in each experiment.

	Gas consumed	Oxygen consumed.	Carbonic acid formed	Diminution of bulk, Carbonic Acid included.
1	11.48	9.50	4	20
2	9.93	10.07	7	20
3	8.68	13.32	8.5	22
4	10.43	11.57	8	23
5	11.68	10.32	7	22
	10.44	10.96	6.9	21.4
	100	104.98	66.1	204.98

The oxygen consumed in these experiments was greater than in the preceding. The proportion of carbonic acid is apparently less, because the experiments were made over water, and the bulk was more diminished by the combustion than in the former case. When they were repeated over mercury, I obtained an average of 8.5 measures of carbonic acid gas from the preceding proportions of inflammable gas and oxygen, which gives us 81.4 measures of carbonic acid gas for 100 of the gas from peat consumed.

The mean of these experiments and the former gives us nearly 102 measures of oxygen consumed, and 81 measures of carbonic acid formed, for every 100 measures of pure inflammable gas burnt; and these proportions I consider as approaching as near precision as we can expect to go, according to the present mode of experimenting.

11. Having

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11. Having thus ascertained the properties of the gas from peat, we may easily determine whether the opinion by Mr. William Henry, be well founded, namely, that this gas is a mixture of the inflammable gases with which we are already acquainted.

Of the four known inflammable gases, namely, the olefiant gas, carbureted hydrogen, carbonic oxide, and hydrogen, of which alone, from its properties, it can be a mixture, we must exclude the first, because the bulk of the gas from peat is not sensibly diminished by oxymuriatic acid Only three hypotheses, then, can be formed; namely, 1st, that it is a mixture of carbureted hydrogen and carbonic oxide; 2d, that it is a mixture of carbonic oxide and hydrogen; or, 3d, that it is a mixture of these three gases all together. Let us examine these hypotheses.

According to the first hypothesis, our gas is a mixture of carbonic oxide and carbureted hydrogen.

The specific gravity of carbonic oxide is . .	$9560=a$
carbureted hydrogen	$6000=b$
gas from peat	$8128=c$

Let these numbers respectively be denoted by the letters a, b, and c, and let the portion of carbonic oxide in the mixture be x, and that of carbureted hydrogen, y; then, by a well-known property of fluids, we have $x : y :: c—b : a—c$. Hence, since $x + y = 100$, we obtain $x = 59.78$ and $y = 40.20$; so that if the gas from peat be a mixture of these two gases, it must be composed of

Carbonic oxide . . .	60
Carbureted hydrogen	40
	100

Now, 60 measures of carbonic oxide and 40 of carbureted hydrogen, when burnt, combine with the following proportions of oxygen, and form the following proportions of carbonic acid; and the mixture undergoes the following diminution of bulk.

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	Oxygen consumed.	Carbonic Acid formed.	Diminution of Bulk.
60 Carbonic Oxide	27	54	33
40 Carbureted Hydrogen	80	40	80
Total . .	107	94	113

The proportion of oxygen required by this supposition, does not differ much from that consumed by the gas from peat; but the carbonic acid is more than is formed by the gas from peat. The diminution of bulk is too small. And, upon the whole, the differences are greater than can be ascribed to errors in the experiments.

(2) According to the second hypothesis, the gas from peat is a mixture of carbonic acid and hydrogen.

The specific gravity of carbonic oxide . . $9560=a$
hydrogen . . . . . $0843=b$
gas from peat . . . $8128=c$
Proportion of carbonic oxide in the mixture $=x$
hydrogen . . . . $=y$

We have, as before, $x:y::c-b:a-c$. From this we obtain, as before, $x=83.57$ and $y=16.43$. So that, if this hypothesis be true, the gas from peat must be a mixture of

carbonic oxide	83.5
hydrogen gas	16.5
	100

The following table shews the oxygen consumed, the carbonic acid formed, and the diminution of bulk, when such a mixture is burnt with the requisite quantity of oxygen.

	Oxygen consumed.	Carbonic Acid formed.	Diminution of Bulk.
83.5 Carbonic Oxide	37 5	75.1	45.9
16.5 Hydrogen	8.5	0.	25.0
Total . .	46.0	75 1	70.9

Here

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Here the diminution of bulk is very different from the truth, while oxygen consumed does not amount to half the real quantity. This hypothesis, then, is still less admissible than the former.

(3) The third hypothesis only remains to be examined, according to which, our gas is a mixture of carbonic oxide, carbureted hydrogen and hydrogen.

It is obvious that, according to this hypothesis, the quantity of carbonic oxide present in 100 measures of the gas from peat, can never be less than 60 measures, nor greater than 83; that the carbureted hydrogen can never amount to 40 measures, nor the hydrogen to 16. But within these limits there is an infinite number of proportions of these gases, which will produce a gas having exactly the specific gravity of the gas from peat. If, however, we make the supposition, which will be sufficiently precise for our purpose, that one of the gases shall always be present in the mixture, in such proportions as to constitute a whole number of measures, then the number of such mixture becomes limited. Thus, if we pitch upon carbonic oxide as the gas which must make a whole number of measures, then the number of mixtures will scarcely exceed 20. But it is needless to examine the products of the combustion of such mixtures, because none of them approach the properties of the gas from peat so nearly as the mixture of carbonic oxide and carbureted hydrogen. The following are a few examples.

		Oxygen consumed.	Carbonic Acid formed.	Diminution of Bulk.
Carbonic Oxide	63	28.35	46.70	34.65
Carbureted Hydrogen	34.76	69.52	34.76	69.52
Hydrogen	2.24	1.12	0·	3.36
Total . . .	100	98.99	91.46	107.53

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		Oxygen consumed.	Carbonic Acid formed.	Diminution of Bulk.
Carbonic Oxide	65	29.25	58.5	35.75
Carbureted Hydrogen	31.5	63.00	31.5	63
Hydrogen	3.5	1.75	0.	5.25
Total . . .	100	94	90	104

If we were to examine all these mixtures in succession, we should find that their properties deviate more and more from those of the gas from peat, as the proportion of carbonic oxide increases, and that the mixture nearest the gas from peat is that in which there is a minimum of carbonic oxide, and of course, in which the hydrogen disappears altogether; that is to say, it is the mixture of carbonic oxide and carbureted hydrogen already examined. Thus, the presence of pure hydrogen gas, in the gas from peat, cannot be admitted; indeed, the evolution of it from a vegetable substance exposed to heat, is contrary to all analogy. But I own I was very much inclined, from the result of the preceding investigation, to consider the gas from peat as a mixture of carbonic oxide and carbureted hydrogen, and to ascribe the differences between the gas which I examined and such a mixture, to errors into which I had fallen in making the experiments. Accordingly, I repeated the experiments day after day, on purpose, if possible, to make them tally with the hypothesis. But as the result of all the trials was constantly the same, I was obliged to renounce it. Afterwards, I satisfied myself by a set of experiments, to be detailed immediately, that the hypothesis, independent of errors of experiments, is inadmissible.

12. The gas from peat, then, not being a mixture of any known gases, we must either admit it as a peculiar compound gas, different from every other previously known, or at least as containing a mixture of a peculiar and hitherto unknown gas. The first of these opinions may be admitted at present provisionally, till a more complete investigation of the inflammable gases from vegetables enable us to decide whether the second be possible.

Let

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Let us endeavour, then, from the preceding experiments, to ascertain the constituents of this new gas. The reasoning from which these constituents are deduced, is founded on a hypothesis not yet strictly demonstrated, though sufficiently probable to be admitted by chemists: the hypothesis is, that when a mixture of the inflammable gas and oxygen are burnt, all that portion of both which disappears is converted into water and carbonic acid. The proportion of carbonic acid formed is known from the experiments, while the proportion of water is deduced from it in the following manner: When oxygen gas is converted into carbonic acid, its bulk is not sensibly altered; therefore, the quantity of carbonic acid formed being subtracted from the quantity of oxygen consumed, leaves a remainder of oxygen gas which entered into combustion, but did not form carbonic acid. It is presumed that the remainder went to the formation of water. It must, therefore, have combined with the hydrogen contained in the inflammable gas. Now, to obtain the weight of this hydrogen, it is only necessary to know, that when oxygen is burnt with hydrogen, it combines with very nearly twice its bulk of that inflammable gas.

Having thus obtained the quantity of carbonic acid and of water, formed by the combustion of the gas, as the carbon in the one and the hydrogen in the other were furnished by the inflammable gas, while the oxygen was furnished by the oxygen gas present, we add the weight of that carbon and hydrogen together, and compare it with the weight of the inflammable gas consumed. If the two weights are equal, we conclude that the inflammable gas was composed of the proportion of carbon and hydrogen obtained by the experiments. But if the weight of the gas be greater than that of the carbon and hydrogen, we are obliged to have recourse to a new hypothesis, and to suppose that the difference of weight is owing to a portion of oxygen and hydrogen present in the gas, which combined during the combustion, and formed water. The proportion of these two substances deduced from the hypothesis, is added to the hydrogen and carbon previously obtained: thus making up the whole weight of gas, and giving us the constituents.

From this account of the mode of analysing these gases, it is obvious, that it is liable to some degree of uncertainty. But the

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the present state of chemical science does not admit of any thing more precise; for, deducing the proportion of carbon from the carbonic acid formed, I consider it as amounting to 0 28 of the weight of that gas. For the experiments of Lavoisier and Smithson Tennant appear to me much more precise than those of Morveau, which, indeed, are contradicted by the more recent experiments of Berthollet, and were not made in such a way as to be susceptible of very correct results.

As the gas employed in the preceding sets of experiments differed a little in its specific gravity, we cannot take the mean result of both. If we take the last set, we have 100 inches of the gas equal in weight to 25 grains, consuming 105 inches of oxygen, and producing 81.4 inches of carbonic acid.

81.4 inches of oxygen formed carbonic acid
23.6 went to the formation of water, and combined with about 47 2 inches of hydrogen, supposing it in the state of gas.

81.4 inches of carbonic acid contain of carbon		10.6 grs.
47.2 inches of hydrogen weigh	—	1.2
Total		11.8
Weight of 100 inches of the gas	—	25.02
Deficiency — —	—	13.22

These 13.22 grains we suppose to have been oxygen and hydrogen present in the gas, and which combined to form water during the combustion. But water contains very nearly 1·7th of its weight of hydrogen. Hence, they are composed of

11.02	oxygen
2.20	hydrogen
13.22	

These being added to the 11.8 grains formerly obtained, give us, for the constituents of the gas from peat,

11.02	oxygen
10.60	carbon
3.40	hydrogen
25.02	

or,

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or, per cent. 44 oxygen
42.4 carbon
13.6 hydrogen

100.0

As this gas contains three constituents, we may give it the provisional name of oxycarbureted hydrogen, till future experiments determine whether it be a mixture or a chemical compound.

13. The gas employed in the preceding experiments, though its specific gravity varied a little, was, however, pretty nearly uniform in that respect. But, in the course of my experiments on peat, I obtained portions of inflammable gas which differed very much, both in their specific gravity and in their other properties, from the gas which we have just examined. I select the following experiments as the most striking that occurred.

The peat was distilled slowly in a small iron bottle. The gas which came over was received in two different jars. The first portion that came over was found to be a mixture of

75 inflammable gas
25 common air

100

Its specific gravity was only ·7274; which gives for the specific gravity of the pure inflammable peat 0.6365. Hence, 100 cubic inches of it, at 60°, weigh only 19.73 grains.

The second portion which came over was found to be a mixture of

71.7 inflammable gas
18.3 common air

100

Its specific gravity was 0.6883, which gives us, for the specific gravity of the pure inflammable portion 0.6089. Thus the two portions of gas differed from each other in their specific gravity and both of them were much lighter than the gas previously examined. Indeed, they approached very nearly to the specific gravity of pure carbureted hydrogen.

With the first portion of inflammable gas thus obtained, I made the following experiments. The oxygen used contained 4.5 per cent. of azote.

From

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	Measures of Gas.	Measures of Oxygen.	Residue after Combustion.	Ditto, washed with Lime-water.	Nitrous Gas added.	Residue.
1	20	20	23	19	46	41
2	20	40	42	37	60	33
3	20	60	61.5	55	118	63.5

From these experiments we can easily deduce the following table.

	Measures of Gas.	Measures of Oxygen	Measures of Azote.	Residue.	Carbonic Acid.	Residual Gas.		
						Oxygen.	Azote.	Gas
1	15	20.1	4.9	23	4	8.84	4.9	5.26
2	15	39.2	5.8	42	5	23.57	5.8	7.63
3	15	58.3	6.7	61.5	6.5	40.34	6.7	7.96

It is curious that, in these experiments, the whole of the gas was never consumed—a proof that the combustion is most complete, when a considerable quantity of azote is present. It is indeed possible, though not probable, that the constant residue was incombustible. We have no means of verifying this by experiment. From the preceding table we deduce the following, which exhibits the proportion of gas and oxygen consumed, and of carbonic acid formed.

	Gas consumed.	Oxygen consumed.	Diminution of bulk, including Carbonic Acid.	Carbonic Acid formed.
1	9.74	11.26	21	4
2	7.37	15.63	23	5
3	7.04	17.96	25	6.5
Average	8.05	14,95	23	5.17
Average per cent.	100	186	286	64.30

Here

Experiments and observations on the inflammable gas from peat.

Here the proportion of oxygen consumed increased with the proportion present. The average result is very different from that obtained in the former experiments; since here 100 of gas consumed 186 of oxygen, whereas, in the former case, the gas consumed only its own bulk of oxygen. The proportion of carbonic acid gas is too small; but over mercury it amounted only to 70 for the hundred of gas.

Here 70 inches of oxygen went to the formation of carbonic acid, and 116 to that of water. These last must have combined with what was equivalent to 232 inches of hydrogen.

70 inches of carbonic acid contains of carbon	9.11 grs.
232 inches of hydrogen — —	6.03
	15.14
Weight of 100 inches gas	19.73
Residue — —	4.59

This residue must be water, and composed of

0.65 hydrogen
3.94 oxygen

4.59

Hence the gas is composed of 9.11 carbon
6.68 hydrogen
3.94 oxygen

19.73

or per cent of 46 carbon
34 hydrogen
20 oxygen

The great difference between this gas and the preceding consists in the diminution of the oxygen and the increase of the hydrogen.

Now, this gas cannot be a mixture of carbonic oxide and carbureted hydrogen: its specific gravity approaches too nearly to that of the latter gas, to admit any notable quantity of the former. It cannot be carbureted hydrogen, because the proportion of carbonic acid formed during its combustion is too small to admit of that supposition.

With the second portion of inflammable gas, which had a smaller

Experiments and observations on the inflammable gas from peat.

smaller specific gravity than the first portion, the following experiments were made.

	Measures of Gas.	Measures of Oxygen.	Residue after Combustion.	Ditto, washed with Lime-water.	Nitrous Gas added.	Residue.
1	20	20	15	9	27	27
2	20	40	36	29	55	
3	20	60	56	47	113.5	59

From these experiments we obtain the following table.

	Measures of Pure Gas.	Measures of Pure Oxygen.	Measures of Azote.	Residue.	Carbonic Acid formed.	Residual Gas.		
						Oxygen.	Azote.	Gas.
1	16.34	19.83	3.83	15	6	1.2	3.83	3.97
2	16.34	38.93	4.73	36	7	20.80	4.73	3.97
3	16.34	58.03	5.63	56	9	37.40	5.63	3.97

It is remarkable that, in these experiments, the residual gas was always the same. This renders it probable that it was incombustible, and that it differed in its nature from the gas which was consumed. The following table exhibits the quantities of gas and oxygen consumed, and of carbonic acid formed, in each experiment.

	Gas consumed.	Oxygen consumed.	Diminution of bulk, including Carbonic Acid.	Carbonic Acid formed.
1	12.37	18.63	31	6
2	12.37	18.13	31	7
3	12.37	21.63	34	9
Average	12.37	19.46	32	7.3
Average per cent.	100	158.7	258.7	59.01

Here

Experiments and observations on the inflammable gas from peat.

Here the quantity of oxygen consumed is less than in the preceding experiments. On repeating the combustion over mercury, I obtained 60.63 as the proportion of carbonic acid from 100 gas consumed.

Here 60 inches of oxygen went to the formation of carbonic acid and 97 to the formation of water. These last must have combined with what was equivalent to 194 inches of hydrogen gas.

60 inches of carbonic acid gas contain of carbon	7.81 grs.
194 inches of hydrogen weigh —	5.04
Total	12.85
Weight of 100 inches of gas	18.85
Residue	6

This residue must be water, and composed of

5.15	oxygen
.85	hydrogen
6.00	

Hence, the gas is composed of

7.81	carbon
5.89	hydrogen
5.15	oxygen

or, per cent. of

41.45	carbon
31.25	hydrogen
27.30	oxygen
100.00	

These experiments were not, perhaps, sufficiently numerous to ensure results that can be altogether depended on; yet, as they were made with all possible care, and some of them repeated two or three times, the errors, I think, cannot be very great.

It is obvious that this inflammable gas, especially the last portion, cannot be a mixture of carbonic oxide and carbureted hydrogen, as its specific gravity is but very little greater than the lightest of these gases. It cannot be carbureted hydrogen, because it neither consumes so much oxygen, nor forms nearly so much carbonic oxide. But as the gas from peat varies in its specific gravity and in its other properties, it is not improbable that it is a mixture of two gases which vary in their proportions. One of them may be carbonic oxide; but I think I

Experiments and obervations on the inflammable gas from peat.

have demonstrated that the other must be a gas with which we are still unacquainted, in a separate state. It must be specifically lighter than carbureted hydrogen; must contain less carbon and more hydrogen. Were we to suppose it a species of carbureted hydrogen, it would not be difficult to deduce its specific gravity and its constituents, from the preceding experiments. But if oxygen enters into its composition, as is by no means improbable, the preceding experiments do not furnish us with the requisite data. At any rate, it would be premature, at present, to enter upon any such investigation till a greater number of the inflammable gases yielded by vegetables be examined.

II.

Observations on Professor Leslie's Theory of Caloric. In a Letter from Dr. HALLIDAY, *of Halesworth,*

To Mr. NICHOLSON.

SIR,

Whether radiant caloric be propagated with great velocity.

AFTER reading Professor Leslie's very excellent treatise on heat, I confess I became a convert to his ingenious Theory of Radiant Caloric, and then instituted a few experiments, not much different from his, with the view of confirming my opinions still more. These experiments, however, which I fear were not performed with *too much* accuracy, have somewhat shaken my faith in the Professor's theory; and I am now more than ever convinced of the truth of the generally-received notion, viz. "that caloric is capable of being projected in right lines with great velocity, and that these lines obey nearly the same laws of motion as the rays of light." I shall not enter into a detail of the experiments at present, but merely state their results, and the reasons why I have been induced to alter my sentiments; and I shall only advert to those from which Mr. Leslie has inferred the theory he has formed of radiant caloric.

Professor Leslie's Experiments.

Mr. Leslie found, that when a screen of tinfoil or even gold-leaf, which is 600 times thinner than the tinfoil, was interposed between the thermometer and the most powerful radiating surface of the heated vessel, the effect on the thermometer was completely intercepted. But that a pane of glass only intercepted four fifths of the caloric, while a sheet of paper did not intercept so much; and, in order to do away the supposition, that the effect produced on the thermometer in the experiments with the glass and paper, was owing to part of the radiant caloric passing through their substance, he observes, that this effect was only produced when the screens were placed about two inches from the heated surface, and that when about a foot from the tin vessel, the rise in the thermometer was not one thirteenth of what it was in the first position. Hence he concludes, that the calorific influence is completely arrested, and that the screen, by this, acquires heat, and, in its turn, displays the same energy as if it had formed the surface of a new canister of the corresponding temperature.

Facts and observations, to shew that radiant caloris passes through bodies.

Now, Sir, in repeating these experiments, I observed the same results, but was led to somewhat different conclusions. I conceive that the screens of glass and paper do not entirely arrest the radiant caloric, but that they allow part of it to pass, and I do so for this reason. When the screen was placed two inches from the heated surface, I observed it acquired heat not only from the rays which it had arrested, but also by *communication*; when I placed it about a foot from the canister, it had not its temperature varied at all, and therefore I conceive the effect upon the thermometer was produced *wholly* by the rays of caloric which passed through its substance.

Heat transmitted through bodies without their temperature being raised

In the first instance, the glass screen received caloric, not only by radiation, but still more by communication from the heated surface; so that its temperature was raised, and it became capable of radiating in its turn; of course, the rays from the canister which passed through the screen, assisted by those from tha screen itself, produced a greater effect on the thermometer. But, in the second case, the screen received no caloric by *communication*, its temperature was not raised, therefore it could not, as Mr. Leslie would have it, " display any energy in causing a fluctuation, or partial swell, in the mass of air, so as to transport the heat. I was anxious to ascertain whether or not the pane of glass, when placed about a foot distant from the heated surface, did acquire any increase of temperature; but I assure you, if so, I could not discover it by a very delicate air thermometer: so that I conclude, that the effect produced on the thermometer, in the focus of the reflection, was by the calorific rays which passed through the screen.

Experiment with a sheet of ice.

I consider Mr. Leslie's experiment with the sheet of ice as establishing nothing whatever; for here the rays are not only arrested, but absorbed; and though I am inclined to believe that some of the rays are transmitted, yet that the "frigorific rays," if I may be allowed the expression, for the sake of being understood, are more than able to counteract any effect which they could produce.

Remarks on the experiment with glass.

In the last experiment which I shall at present notice, and which Mr. L. regards as the *experimentum crucis,* I think he does more to establish the fact, that part of the radiant caloric does pass through glass, than to make good his own theory; for here we see the effect produced, when there is a certainty that none of the radiant caloric can pass, and we find that this effect is less *cæteris paribus,* by two degrees, than when there was a possibility that some part of it might pass; and if we compare that with the quantity which passed through the glass in the former experiment, we shall find that they are nearly equal; and as metalic surfaces reflect the whole of the radiant caloric, I conceive there is but little difficulty in accounting for the striking difference which he observes took place when the tin coatings of the panes of glass were on the outer side. I admit, with Mr. Leslie, that the calorific emanation is incapable of permeating solid substances which are opaque; but when *light* can pass through, I am inclined to believe that *radiant caloric* is also capable of finding its way, or, in other words, that radiant caloric is capable of passing through transparent solid substances.

General Conclusion.

Sir, I have ventured to trouble you with these rather puerile observations, with the view of drawing some of your correspondents to the subject. It is a field in which much may yet be done.

I am, Sir,
Your very obedient Servant,
ANDREW HALLIDAY, M. D.

III.

Description of a Drag for raising the Bodies of Persons who have sunk under Water. By Dr. Cogan, *of Bath* *.

SIR,

Premium for a drag.

FROM the Reports of the Royal Humane Society for the year 1805, I learn, that a premium is offered by the Society instituted in London, for the Encouragement of Arts, Manufactures, &c. "To the person who shall invent and produce to the Society a cheap and portable drag, or other machine, superior to those now in use, for the purpose of taking up in the best and most expeditious manner, and with the least injury to, the bodies of persons who shall have sunk under water:" and accordingly I beg leave to submit to the inspection of the Society two models.

Account of the drags used in Holland, &c.

I have long, Sir, been discontented with the construction of the drags which have hitherto been in use, both in this and in other countries. Those used in Holland are not more than three or four inches in diameter, with very long and sharp points. They cannot therefore be properly applied to a naked body; and were not the Dutch sailors and boatmen, who are most exposed to danger, very thickly clad, they might be productive of mischief. I attempted, when resident in that country, to make some improvements, by turning the points obliquely inwards, so as to catch the clothes without penetrating deep into the body; but still these were only applicable in cases where the subject fell into the water in his clothes. The drag which is now used in London is, in many respects, exceptionable; it is clumsy and dangerous.

History and

The design of establishing a Humane Society at Bath, induced me to reconsider the subject with more attention, and the result has been the construction of two drags, according to the models which are sent to you, at the desire of that Society. The consideration of economy has induced me to construct the drag, Fig. 2. as it may be made at about half the price of the other, and, in some cases, be equally useful. The drag, Fig. 1. is applicable to every case, and the only objection to it is its higher price.

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* For which the Society of Arts gave the Gold Medal, 1806.

description of the instrument. You will perceive, by the annexed drawing, the object in view, which is to multiply the chances of laying secure hold of any part of the body, without the possibility of an injury. Had the dimensions been smaller than they are, the drag would not encompass every part of a human body; and without the partition and curvatures at the extremities, the distances would be too great, and the body of a child might fall through the intermediate spaces. By means of the sliding hooks at the ends, the instrument is adapted both to naked bodies, and those which are clothed. As bathers are naked, the sharp-pointed extremities might lacerate in a disagreeable, though not a dangerous manner: or, by entering the skin, they might impede a firmer hold. They are therefore made to recede.

But in accidents from skaiting, or in such where the subject falls into the water with his clothes on, the hooks will be of the utmost advantage, as the slightest hold will be sufficient to render the body buoyant.

The upper extremities are made both with a socket and a loop, by which they are accommodated either to a pole or a cord; or, which is still better, to both. In ponds or rivers, where accidents are most likely to happen, should they occur at a distance from the shore, no pole would be able to reach to a sufficient extent, unless the assistants were in a boat, which is not at all times at hand. In such cases a cord may be attached to the loop, and the instrument be thrown to the place where the body is supposed to lie. If the person exposed to danger should be able to swim a little, or in any way just support himself from sinking, he might possibly lay hold of the floating piece of wood, connected with the lower end of the drag by means of a rope, and thus be brought to shore. This appendage answers another purpose. In rivers particularly, the limbs of the instrument may probably catch roots of trees, &c. and can only be disengaged by pulling the drag in a contrary direction, by means of the floating wood and rope.

Description of a drag to raise the bodies of drowning persons.

When I said that both pole and cord are preferable to either singly, it was for the following reason. I have found, by experiments, that a cord tied to the ring or loop, and passing through a hole made at the upper end of the pole, gives a double advantage. The drag, with a pole attached to it, of not more than 10 or 12 feet in length, may be projected several yards further than without it; and in drawing forward the drag, till the end of the pole is brought within reach of the hand, the subject may be raised above the surface of the water in the most proper direction. But a pole of 15 or 16 feet in length is unwieldy, and would even float the drag, unless it was made much heavier.

If a drag was wanted in those cases only, where it is not necessary to throw it to a distance, then Fig. 2. would answer every purpose. It is obvious that this requires a pole to be fixed in it, so that the hand may direct the projecting parts to the body, which otherwise could not always be done.

We have not as yet had an opportunity of trying these drags upon a human body; but upon an effigy made in every respect as like as possible in form to the human body, both clothed and unclothed, they have answered in the most satisfactory manner. The effigy was brought to the surface in various directions, without once slipping from the hold.

I shall just beg leave to add, that with two drags and a boat, assistance given in time would almost ensure success. A hook catching a single thread, it is well known, will be sufficient to bring a human body to the surface of the water, or till it becomes visible: a second drag at such time might be applied to any part of the body, so as to secure a firm hold.

The workman charges the triangular drag at one guinea, the other at 12 shillings. A pole 16 feet in length was charged three shillings. The fangs were estimated at one shilling and sixpence.

I am, Sir,
Your most humble Servant,
THOMAS COGAN.

Bath, March 1, 1806.
To C. Taylor, M.D.

Reference to the Engravings of Dr. Cogan's Drag. Plate VIII. Fig. 1, 2, 3, and 4.

Fig. 1. A shows the drag complete, with two cords, B and C, attached to it; that at the top, B, is fastened to a ring at D; the bottom cord is tied to a hole in the iron at E. The six ends of the projecting branches have each a barbed claw, which can be slided forward or drawn back, as may be thought necessary. There is a hollow socket in the upper part of the drag at D, so as to admit the end of a pole to be screwed therein, whenever it may be thought useful.

Fig. 2. Is the cheaper or more simple drag, and intended only to be used with a pole G, fastened in its hollow socket by the screw H, and to be used in the manner of a rake, to bring the body to land. It has barbed claws at the extremities of its branches L L, moveable backwards and forwards, which claws slide in a groove made in the extremity of each branch.

Fig. 3. Shows one of the claws drawn upon a larger scale, screwed to one of the extremities of a branch. In this situation the screw head appears as I, on the outside of the branch, and the claw is within, and does not extend beyond the extremity of the branch.

Fig. 4. Shows the same barbed claw as its utmost extent, projecting beyond the extremity of the branch. The end of the worm of the screw, which holds it fast in that position, appears at K.

IV.

Arguments against the Volcanic Origin of Basalt, derived from its Arrangement in the County of Antrim, and from other Facts observed in that Country. By the Rev. William Richardson, *late Fellow of Trinity College, Dublin.*

Celebrare domestica facta.—Horat.

Facts and observations respecting the basalt in the county of Antrim; adduced to show that it is not volcanic.

I HAVE, in the preceding parts of this Memoir *, discussed most of the arguments that have been adduced, by different writers, to support the volcanic origin of basalt: and I have examined the facts stated by them, to try how far they apply to this question.

I now return to my own country, which seems more copiously furnished with curious basaltic facts than any of those upon which foreign writers have dwelt so much.

The question (to us at least) is important; for it is the origin of the ground we live upon that we are inquiring into: every particle of the surface of an extensive basaltic area, having merely a thin coat of most fertile earth, slightly covering basalt strata, accumulated upon each other to a great height; and most frequently, as it were, bursting through this surface, and displaying, in perpendicular façades, the arrangement of the materials that support us.

Whether these materials, so arranged, be formed by the hand of nature, in her original construction of the world; and thus our basaltic strata (in the language of naturalists) be entitled to the appellation of *primary*: or whether this construction of our country is to be considered as produced by mighty agents, covering our quondam surface with new and *secondary* stata, poured forth from the bowels of the earth, is surely an interesting question in the natural history of our country. And as every writer who has taken up the question of the volcanic origin of basalt, and maintained the affirmative, has recurred

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* From the Irish Transactions, Vol. X. The two former parts consist of An Examination of Desmarest's Memoir in the Acad. Par. 1771, and of the principal philosophers who have followed his theory.

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to the county of Antrim for proofs, I hope that I too will be allowed to extract, from the same source, such proofs as appear to me to support the negative.

In discussing this question I shall abstain from all arguments *a priori*, and limit myself to *facts* alone; of which I hope to lay before the reader several that have escaped the notice of my predecessors; feeling that I ought to make him some amends for having detained him so long in a barren discussion of opinions, and an uninteresting detection of misrepresentations.

Before I proceed to compare the circumstances in which our basaltic area resembles or differs from volcanic countries, I must answer a charge that has been brought against me. I have been told, that it is presumption in me, who never saw a volcano, to take up a question, the solution of which must depend upon an intimate knowledge both of basaltic and volcanic countries.

I first plead example; as not one of my predecessors, who have written upon this topic, has (so far as I can find) examined both volcanic countries and our basaltic one.

I have also authority for saying, that an examination of existing volcanos is not very instructive. Mr. Kirwan tells us Collini twice ascended Vesuvius, and witnessed its eruptions, but complains he got no knowledge by it. Mr. Ferber's testimony is exactly similar. And, indeed, it is plain that, in an eruption, the lighter materials first projected upwards; then falling down, and accumulating upon the weightier, that had flowed in lava, must make it very difficult to trace arrangement; and this is the surest guide in all questions relative to cosmogony.

Mr. Strange's observations on this topic are amusing: he lets out the secret without knowing it, or availing himself of it. He says, " The phænomena of recent volcanos are very " little calculated to give us instruction. A few days tour in " Auvergne, Velay, or the Venetian State, are worth a seven " years apprenticeship at the foot of Vesuvius or Ætna."

Mr. Strange was not aware, that Auvergne, Velay, and the parts of the Venetian state he alludes to, were originally basaltic countries, in which, afterwards, volcanos erupted. Here he found a rich variety of materials: for, besides the common volcanic substances, he found all the varietes of basalt, with the matters that usually accompany them, *ochres, zeolites, chalcedonies,* and *calcareous spar;* while at Ætna and Vesuvius he met with burnt matters alone.

Facts and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

The points of view in which I shall compare volcanic countries, as described by the most accredited writers, with our basaltic district, so often referred to by the same authors, are:

First. The prominent features and general resemblance.

Secondly. The different arrangement of the materials in volcanic, and our basaltic countries.

Thirdly. Frequent change in the arrangement of the materials in our basaltic country.

Fourthly. Striking and radical differences between our basalt strata, and all known currents of lava.

Fifthly. Substances found imbedded in our basalt, and never in lava.

Sixthly. Different effects produced upon foreign substances (particularly calcareous), when coming in contact with basalt and with lava.

Seventhly. Divisibility of the mass into regular forms, essential to basalt, but never noticed in lava.

First. The general and leading features of volcanic countries are admitted to be *isolated mountains,* generally conic, *truncated cones, vast craters,* with *currents of lava* issuing from them, which may be traced many miles. But as all writers upon this topic candidly admit that we have nothing similar in this country, I will not press the argument, nor enquire whether their modes of accounting for the want of these features be satisfactory or not.

Secondly. If basalt be lava, and (as this theory supposes) once flowed from a volcano, we should expect to find it arranged in the same manner with the currents of lava, which are contiguous to most known volcanos. But here the difference is most striking: for, while all writers that describe volcanic countries, represent the ejected matters as confusedly arranged, and altogether a heap of disorder; with us we observe, in the disposal of our basalt, the most consummate regularity; every separate stratum preserving steadily its own place, and never breaking into that of another.

Besides, most writers admit that currents of lava are never parallel to one another: while our basalt strata, accumulated

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mulated upon each other, preserve the most steady parallelism.

When we compare our accumulations of basalt strata with accumulations of currents of lava, which have been heaped upon one another by successive eruptions, we observe a most important difference. Currents of lava have always a layer of vegetable earth between them: this is admitted by all parties. For, while those who wish to impeach the chronology of Moses, make a prodigious interval between the eruptions necessary for the formation of this layer of earth, Moses's advocates prove, from facts, that it is often formed in a much shorter time.

Interposing layers between currents of lava being thus established, we are to look if any thing similar can be observed between basalt strata: but no such thing is to be found. Our basalt strata, whether of the same or of different varieties, pass into each other *per saltum*, without interrupting the solidity of the mass, or without exhibiting a particle of extraneous matter between them *.

Thirdly. I observed, in a former Memoir, that, on our basaltic coast, nature changes her materials, and the stile of her arrangement, every two or three miles; a fact which opposes insurmountable difficulties to the position, that the basalt strata, forming this coast, are of volcanic origin. I will select two or three of these numerous little systems, and state the order in which the strata are arranged in each of them, in a vertical direction, to give the advocates for their volcanic origin an opportunity of exerting their ingenuity, by showing how they manage their volcanos, to make them produce such diversified effects.

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* I am aware that the ochreous layers, or strata, lying between our greater basalt strata may be stated, as contradicting this position.

The nature of these ochres (common to all basaltic countries) has given rise to much controversy; which, were I to enter into now, I would be led too far from the present question. But as this fossil makes a most conspicuous figure in many parts of Antrim, I think it well entitled to a place in the *statistical survey* of that country; the basaltic part of which I have undertaken to oblige my friend, Mr. Dubourdieu.

On the present occasion I shall only say, that I accede to the conclusion which Mr. St. Fond adopted, after long doubt, and much puzzling; to wit, "*That these ochres were pure basalt, altered by some chemical opera-*" *tion of nature, with which we are unacquainted.*"

Facts and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

From Dunluce to Seaport, the façade (here the base of the arrangement) is composed of strata of tabular basalt; upon which are accumulated, up to the summit of Dunmull, columnar strata, mixed with others, of the variety called *irregular prismatic.*

East from Carrickarede, the base of the façade is white lime-stone; upon which, as long as it continues perpendicular, we find ochreous and columnar strata alternating; while the hill of Knocksoghy, above, is an uniform alternation of columnar and irregular prismatic.

The strata, forming the promontory of Bengore, are more irregularly mixed: six of tabular basalt, five columnar of four different varieties, three ochreous, and two irregular prismatic, sixteen in all: of which, after the tabular that forms the base, no two of the same kind are contiguous to each other.

The volcanist will see that he must find a distinct volcano for every separate little system surrounding our area; and that he must make the same crater emit different varieties of lava, and frequently by alternation.

Fourthly. An examination of our basalt strata, taken separately, and so compared with distinct currents of lava, will, I apprehend, turn out as little favourable to their volcanic origin as the comparison of their masses appear to do.

Whoever has read Mr. Desmarest's Memoir, or even my quotations from it, must admit that, if his theory be well founded, all our basalt strata must have once been currents of liquid lava, and, of course, should resemble those known to have issued from existing volcanos. But, I apprehend, instead of similarity, the most decided differences will be found between them.

Currents of lava, we are told, are always narrower and deeper, in the vicinity of the crater, broader and shallower, as farther removed from it: but our basalt strata are of uniform thickness in their whole extent.

There is another point of view in which the difference between basalt strata and currents of lava is still more decided. Sir William Hamilton, Ferber, Spalanzani, and even Mr. Desmarest himself, informs us, that, in all currents of lava, the materials composing them are invariably arranged, in a regular gradation, according to their specific gravities: thus, at the lowest

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lowest part of the current, compact lava, then cellular lava, then scoria, next cinders, and lastly, volcanic ashes. But, in our basalt strata, nothing similar is observed: the material is uniform; both density and specific gravity the same, through the whole thickness of our deepest strata.

Fifthly. That basalt never was in fusion, appears plainly from the substances found it, and never in lava; and which, from their nature, could not have sustained the heat of a volcano.

Of these, zeolite, chalcedony, and calcareous spar, seem to abound in the basalt of all countries, but never have been noticed in unquestioned lava. The first fuses, and the third calcines, in a very moderate heat; and, though chalcedony be more refractory, yet exposed to a strong heat, it loses its beauty, and the delicacy it exhibits in its natural state. These substances are most copiously dispersed, also, through our basalts; but as this topic has already been often urged, I will pass on to substances peculiar to my own country.

A variety of basalt, found in abundance at Portrush and the Skerrie Islands, is full of pectinites, of belemnites, and, above all, of cornua ammonis: these are dispersed through the whole mass, equally abundant in the interior and on the surface. This basalt vitrifies, and the marine substances it contains calcine in the fire of a common salt-pan; of course, never could have sustained a volcanic heat.

Another fact occurs, which seems decisive against the volcanic origin of basalt. Some varieties of this fossil, contiguous to Portrush and the Giant's Causeway, upon being broken by a sledge, discover, in their interior, cavities, some filled with fresh water, others bearing evident marks of having once contained it. Of these basalts, some were of a different variety from that of the Giant's Causeway, but of similar grain and hardness; others were precisely of the same variety, *columnar, prismatic, articulated* and exactly the same in grain. At the Causeway itself, I never found any; but in some basalts very near it, on the west side, I have met with it: these had fallen from an upper stratum.

A most respectable correspondent, to whom I communicated this fact, as new in natural history, tells me, he suspects the water passed in by percolation. Determined to pay all atten-

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tion to any thing suggested from such high authority, I took my friend, Mr. Joy, to the spot where I used to find the water in the greatest abundance (Ballylagan). We broke several stones, and, where we found water, observed that, at first, it wet the whole fracture evenly; but, as it evaporated gradually, the wet was confined to cracks, diverging from the little cavity that had contained the water. These, therefore, we at first supposed must have been the passages through which the water had made its way: but, on attentively examining the cracks, we perceived that, as they radiated from the cavity, they diminished in breadth, and finally terminated in the solid stone; of course, that the water had not come in by them.

Another fact seems conclusive against percolation. I never found, in our basalt, any cavities but those which contained water, or which bore evident marks of having been once filled with it. We have, therefore, this alternative:

Either the water first made its way through the compact tissue of the basalt, then collected, and dilated itself with such force, as to form rounded cavities, often larger than a pistol-bullet, which, on many occasions, it afterwards forsook:

Or, we must admit the water to have been coeval with the basalt: to which, of course, we cannot ascribe an igneous origin.

Sixthly. As we know the high state of ignition in which lava issues from a volcano, it is reasonable to expect that, when, in its course, it meets with extraneous substances, it should produce upon them such alterations as are the usual effect of intense heat, applied to these same substances. Basalt, likewise, is often found in contact with similar matters. Hence, by a minute examination of these contacts, we have an obvious mode of ascertaining, whether the basalt also had encountered them in the same state of ignition we know the lava did.

As my country, to a great extent around me, is composed of nothing but basalt and lime-stone, I have no other substance but lime-stone upon which I can make observations. This, however, I apprehend, will be found abundantly sufficient to decide the question.

About one hundred yards from the beautiful cavern, called Long

Facts, and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

Long Gilbert, near the eastern extremity of the calcareous façade, a mile from Portrush, we find, half way up the precipice, a vast basaltic rock, inserted in the middle of the lime-stone mass, and, at the contact, so united to the lime-stone, as to form, with it, but one solid mass.

The peninsula of Kenbaan, near Ballycastle, is the spot where basalt and lime-stone come in contact in every possible way. Pieces of lime-stone of all sizes, imbedded in the basaltic mass, and similar fragments of basalt, dispersed in like manner through the lime-stone, and, in the precipice above, strata of basalt, and lime-stone alternating. Here the opportunities of examining the contact of basalt and lime-stone are numberless; and, on every occasion, I found them united solidly; the line of demarcation correct, as if drawn by a pencil; not the least trace of calcination, such as might be expected from the calcareous matter, coming in contact with so glowing a mass, as this theory supposes our basalt to have been.*

This unexpected circumstance has somewhat embarrassed the volcanists; who, to account for it, have been driven to various exertions of their ingenuity: but not one of them seems ever to have inquired what was the result, when calcareous matters came in contact with actual lava, as it flowed. Here an obvious mode presents itself, of deciding the question, whether basalt and lava have a common origin: for, if their contacts with calcareous matter produce the same effects upon it, we have a strong presumption in favour of the affirmative. On the contrary, should the effects turn out to be totally different, we have a conclusive argument in support of the negative.

Whether this mode of bringing the question to issue did not occur to the gentlemen who support the volcanic origin of basalt, or whether they did not like to commit a favourite theory to so rude a test, I will not presume to conjecture. Direct evidence, with a view to the question, I admit I have none; yet, by an attentive examination of different writers on volcanic subjects, I find pretty good light is thrown upon this topic. The evidence I will adduce, is, I confess, indirect, and the mention of the subject incidental; yet I do not, therefore, give it less weight; for, since I engaged in polemic natural history, I have discovered, that a reliance on positive assertion is not the surest mode of obtaining truth.

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* The result of my observations, on the contacts of basalt and lime-stone, perfectly correspond with those of Mr. St. Fond, in Vivarais (Min. des Volcans, chap. 13.) Dr. Hamilton, I admit, saw things in a different point of view: but as he does not refer us to the places where he examined these contacts, I cannot bring the point to issue in my country.

Facts and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

The first evidence I shall produce, to the effect of actual glowing lava upon calcareous substances, is that of Lord Winchelsea, whose letter to King Charles II. (quoted by Sir William Hamilton), giving an account of the great eruption of Ætna, in 1669, says: " Where the streams of lava meet with rocks and stones of the same matter (as many are), they melt and go away with the fire. Where they meet with other compositions (calcareous, no doubt), they turn them to lime or ashes."

Mr. Ferber's testimony on the subject is decisive. He gives us, in his eleventh letter, a catalogue of ejections from Vesuvius; of which No. 6 is, by his account, " white lime-stone or marble, in loose pieces, some burnt and calcined." He observes, " they are found, likewise, in the ashes and lava, and then constantly calcined and farinaceous." Again, letter 14, he says, " at Monte Albano, the lava, as well as the piperino, contain calcined fragments of lime-stone."

Tozzetti di Targioni, in his elaborate account of the mineralogical productions of his own country, confirms Ferber's testimony, as to the uniform calcination of calcareous substances.*

Since, then, glowing lava uniformly calcines the calcareous substances it comes in contact with, and basalt produces no effect whatsoever upon them, are we not to conclude, that it did

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* Tozzetti is full on the subject: He says (page 448, Vol. IX) " Se materiali sieno di natura *vitrescenti*, formeranno lave *vetrine*, se *calcarei* o *apiri*, le formeranno *polverose*."

Page 250. " In essi (lave vesuviane) si vedono misti materie vetrificate, con materie calcinate, e con altri quasi non punto tocche dal fuoco."

Page 252. " Il fuoco volcanico, nelle vescere della montagna di San Fiora, abbia offeso—fuse le massolette di metalli, e calcinate o vetrificate, secondo la loro attitudine altre sostanze."

Facts and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

did not encounter them in a state of fusion? which is the point in question.

Seventhly. Upon the last difference I shall mention between basalt and lava, I must dwell a little longer; both because it seems radical and essential; and also, because it lays open some new and curious facts, relative to basalt, which have hitherto escaped notice.

I allude to that property which all basalt strata, that I ever examined, have, of dividing or separating into regular forms, generally with plain sides. For that this is a principle inherent in the mass, and coeval with its original formation, is obvious, from the striking difference between the plain brown side of the figure and the irregular, granular fracture, generally blue or grey: the former an arrangement of nature, the uniform effect of a cause, with which we are unacquainted; the latter the irregular effect of a violent stroke or impulse.

If the theory we are discussing be well founded, all our basalt strata were once currents of lava, flowing from volcanos. For this we have the authority, or, rather, the assertion, of the founder, and the most accredited supporters of the opinion. In substances, therefore, by their accounts, exactly the same, and of the same origin, (for they use basalt and lava as synonymous terms,) we have a right to expect similar properties; and to look for, in lava, an internal arrangement of the mass into regular forms, conformable to what we meet with in all basalts. But nothing similar has been observed in lava, and the description of the Volvic lava is irreconcileable to this property; for we are told it breaks in all directions, *casse en tout sens:* and Mr. Desmarest himself mentions this, as a mark of distinction between it and the neighbouring basalt.

In distinguishing the varieties of lava, we have a clew to guide us. We know the process by which it was formed; and often, upon inspection, we can discover the original material, the mother stone, by whose fusion it was made. The operation itself, too, enables us to make new distinctions, from the different intensity of heat, and different gradations in cooling.

On the contrary, we get little information from inspecting the fracture of basalt. We can tell that, in some, the constituent materials are more completely blended than in others: which seems the same thing as to say, there is much difference in grain; a great interval between the coarsest and the finest. But all this is by insensible shades; no such thing as drawing lines, by which we can mark the varieties of this fossil. Even where other differences are most essential, between the varieties of basalt, inspection cannot be relied upon. For instance, the siliceous basalt, full of marine exuviæ, passes, by gradation, from a grain as fine as jasper, until it becomes indistinguishable from the Giant's Causeway stone, and even coarser.

Facts and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

If we look to nature for assistance, in classing the varieties of basalt, we will be no longer at a loss: we will find, she has impressed an indelible character on each variety of this fossil; a specific figure, into which every stratum is divisible, in its whole extent, being formed, as it were, by an agglutination of similar figures;* in the same stratum, all of nearly the same degree of perfection; but, when we compare different strata, of the same variety, the perfection or neatness of the work varies, until it passes into an amorphous mass.

Nature seems to have provided, as carefully, for the preservation of the distinctive characters, of the different varieties of basalt, as she has done, to prevent confusion in the several tribes of the animal and vegetable kingdoms. We see our basalts often, by gradation, losing their own forms, but never assuming that of another variety; and, in the last stage of evanescent form, we can trace an effort to preserve their own appropriate figure. This is very observable in our columnar basalt, and in the long horizontal prisms of our whyn dykes.

I can also trace something like a generic difference, between the varieties of our basalt: for some of them have but one principle of construction, to wit, the external visible forms; into which, upon the slightest inspection, they appear to be divided: no internal construction; the fracture irregular, and generally conchoidal. The basalts of this

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* I do not use the word *similar* in a strict mathematical sense; meaning no more than a strong, general likeness, so decided, that the figures of one variety cannot be mistaken for those of another.

Facts and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

this class are, the columnar, the irregular prismatic, and the tabular. I have not been able to discover subordinate forms, or an internal construction, in any of these basalts.

Other varieties, on the contrary, are regularly arranged internally; the large prism breaking into smaller, sometimes to a great degree of minuteness, as in the Portrush silecious basalt. The coarse Portrush Basalt, whose prisms are mostly quadrangular, and the unarticulated pillars of Ballylagan, have likewise the same property, in an inferior degree; while the basalts of our whyn dykes have often their subordinate prisms finished with great neatness.*

But the forms into which our basaltic masses divide, are, by no means, limited to prismatic alone. The pyramid is a common figure in our whyn dykes; and the most perfect joints of the Giant's Causeway pillars, partake both of the prism and of the pyramid, and have also a mixture of curve, and plain surfaces: the latter in number equal to the denominator of the figure; while the former amounts to double that number, plus two. Thus, a pentagon joint, taken from one of our most perfect pillars, has five plain, and twelve curve surfaces; but curve surfaces are irreconcileable, either to *crystallization* or *desiccation*.†

We

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* This subordinate construction is well illustrated, in a drawing of these prismatic stones, taken from a great whyn dyke, now used as a quarry, nearly two miles west from Belfast.

The constituent figure here is a triangular prism, whose angles, at the base, seem double the angle at the vertex.

My ingenious friend, Dr. M'Donnel, to whom I had mentioned the curious construction of our whyn dykes, was so struck when he saw the prismatic stones of which this dyke is formed, extracted from the quarry, that he employed a painter to make a drawing of some of them; and he was so good as to give me a copy.

† The acute angled triangular *pyramids*, which ascend from each angle of the joint, and often reach up to the middle of the incumbent one, have their insides sloped away, in an hyperbolic curve; while the grooves in the lower part of each joint adapted to receive these, with similar curvature, added to the former, make twice as many curve surfaces as the figure has angles. The concave and con-

vex

Facts and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

We have another variety of basalt, whose surfaces external, and, if I be allowed the expression, internal, are all curves: its form is round, and it is composed of concentric spheres, like the pellicles of an onion.

This variety Mr. St. Fond himself admits not to be of volcanic origin. He says (Min. des Volc. page 46), *it must have taken this configuration naturally*. Its mode of arrangement, in the places where it is found, seems still more extraordinary. It is generally imbedded in an indurated basaltic paste; in Mr. St. Fond's language, *incorporée et incastrée dans des massifs de basalte informe*. In this state, it is sometimes built in the form of a wall, of which the globular basalt is the stones, and the unformed the cement.

I have great reason to believe, that the varieties of basalt in other countries are exactly the same as in our own; and that nature has taken the same pains to keep them distinct everywhere.

The columnar basalt, of all countries, corresponds precisely with that of the Giant's Causeway, and our other groups, as appears from the sameness of their curious articulations.

Our irregular prismatic exactly answers the description of

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vex bases add two more; but, by Sir Torbern Bergman's definition, crystals are bounded by *plain surfaces*.

These facts cannot be exhibited in distinct joints; for the cohesion is so strong, that the ascending pyramids invariably break off, as the joints are separated from the pillar. It is the projecting fracture that remains, which gives the joint the appearance of a *mural crown*, as was observed by the early writers on the subject.

The destruction of these ascending pyramids makes the separate joint totally different from what it was, when existing in the perfect pillar.

To illustrate all this, I give a drawing of two pillars: one, as it appears when long exposed to the air, which acts principally upon the joints; while the dilation and contraction, from heat and cold, loosens the pyramids, and separates them from the pillar.

The second pillar exactly represents the state in which they appear, where the mass is lately quarried into, and the air has not had time to operate.

I add some joints in their natural state. This nicety of construction abates, as the pillars graduate through imperfection to an amorphous mass; yet occasional traces of it are long observable.

Facts and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

the basalt incumbent on the columnar at Bolsena, as given by Ferber. It is obviously similar to those at La Trezza and Pont du Baume; and Mr. Mills's view of the isolated basaltic rock at Ardlun (Phil. Trans. 1790), accurately represents this variety, in many façades, near the Giant's Causeway.

Sir Joseph Banks's account of the stratum, incumbent on the columnar at Staffa, might serve for our irregular prismatic, in most places; and the moment I shewed my friend, Mr. Joy, our neat pillars at Craigahuller, he perceived the striking likeness between the stratum, incumbent on them, and that covering the grand colonnade at Staffa.

The slight accounts we have of the Scotch whyn dykes, shew, that they are formed, like our own, of horizontal prisms.

And our globular basalts, with concentric spheres, so curiously imbedded as we find them at Port Cooan, near the Giant's Causeway, and lining some whyn dykes at Belfast Lough, are precisely the same with those taken notice of by Mr. St. Fond, at Ardenne, at Cheidevant, at Montbrul; and also by Mr. Strange, in the Venetian state.

Until the advocates for the volcanic origin of basalt can discover, in lavas, something corresponding to these curious circumstances attending our basalts, can they persist in pronouncing them to be identically the same? The one (if I may be allowed to use the expression) a *factitious* substance, of known and posterior formation; the other, bearing evident marks of the hand of nature, both in its general arrangement in mighty strata, and also in its numerous varieties. For we know that nature delights in diversifying her operations, and in executing what seems to us the same work, in many different ways.

V.

Method of adjusting a Transit Instrument in a Plane of the Meridian. By Sir H. C. ENGLEFIELD, *Bart. M. P. F.R.S. &c.*

To Mr. NICHOLSON.

SIR,

Introduction.

LOOKING over some papers the other day, I found the inclosed, which I had drawn up some years since. I do not know that the method of placing a transit instrument, therein described, has been made public. If it has not, I think its ease and accuracy renders it not unworthy of publication. Should you be of a different opinion, you will be so good as to return it to me.

I am, Sir,
Your obedient Servant,
H. C. ENGLEFIELD.

Tilney-Street, March 11, 1807.

Adjustment of the transit instrument, by levelling its axis, and observing the difference of time between the transit of the pole-star and of another more distant from the pole; the R. A. being given

Let Z (Pl. 8) be the zenith; P, the pole; HO, the horizon; ZPI, the meridian circle; ZK, a circle of altitude distant from the meridian by a small quantity IK (suppose a degree); 1 2 3 4, the diurnal circle of the pole star, whose radius is 1° 45′ nearly; and let the altitude of the pole be 51° 30′. Then, when the pole-star is on the northern meridian, its altitude 3 I, will be 49° 45′, and its zenith distance Z 3, 40° 15′; and let ACD be a part of the diurnal circle of a star whose polar distance is 46° 30′, and N. meridian altitude 5°.

Now, suppose a transit instrument, whose axis is accurately levelled, and of course in the meridian at Z, to point at the horizon to K, instead of I, the true meridian; then, at 3 (the altitude of the pole-star under the pole), it will point at B, and the arch 3 B will be to IK as the cosine of the altitude 3 I to radius; but 3 B, measured on the diurnal circle of the pole-star, will be the sine of its distance from the meridian to the radius P 3 or P B; and as, in small arches, the arch of a

Adjustment of the transit instrument, by levelling its axis, and observing the difference of time between the transit of the pole-star and of another more distant from the pole: the R. A. being given.

great circle, or of a small circle, or their sines, are nearly coincident, we shall have very nearly as Z 3 (the zenith distance) to P 3 (the polar distance), so is the value of 3 B, in degrees of the pole-star circle, to its value in degrees of a circle whose radius is Z 3. And as the radius Z 3 is to P 3 very nearly as 23 to 1, the error of the transit telescope, at the altitude 3 I, will be measured by a scale (if it may be so called) 23 times as great as itself.

Now, let there be another star, A, whose northern meridian altitude is as small as it conveniently can be, for example, 5°, whose polar distance is, therefore, 46° 30′, and whose right ascension is the same as that of the polar star; then, if the transit telescope be in the meridian, both these stars will pass through it at the same time; but if it be out of the meridian by the quantity IK, the star A will pass through it when it comes to C, but the polar-star not till it comes to B, when the star A is got to D, in its diurnal circle.

The value of AC being therefore found, by multiplying IK by the cosine of its altitude AI, that value, being reduced to the angular value to the radius PA, will give the time of the star A passing through the transit telescope, after the time of its passing the meridian; and the same operation being performed for the pole-star as before directed, the difference of these times will be the error in time of the transits, answering to the given deviation IK of the transit telescope. And tables having been previously constructed for such stars as shall be thought convenient, the transit telescope may, in a very short space of time, be set to the meridian, with a degree of precision unattainable by any other method.

If the star A precedes the pole-star in its passage under the pole, no tables are requisite, nor any thing necessary to be known but the exact difference of the right ascension between the two stars; for, having observed the transit of the star A (the instrument being previously brought near the meridian, suppose half a degree), then elevate the telescope to the pole-star, by moving the horizontal adjustment of the axis: keep the pole-star on the middle wire till the due interval of time between their transits is elapsed; the instrument will then be extremely near its true position; and, by repeating the observation once more, will be brought to a perfect exactness. Or, if another star, following the pole-star in its passage, be observed

served on the same evening, if the times elapsed between their transits are equal to the tabular difference or their right ascensions, which will probably be the case, the accuracy of the first placing the instrument will be immediately ascertained. Other stars near the pole may be made use of in the same manner as is here described for the pole-star, but with proportionally less advantage, as the polar distance in increased.

Adjustment of the transit instrument, by levelling its axis, and observing the difference of time between the transit of the pole-star and of another more distant from the pole; the R. A. being given.

It is also obvious, from the figure, that the transit of the pole-star above the pole, may be also used, and that with nearly, though not quite, the same advantage as the transit below the pole.

The same method may also be applied with equal ease, if the second star A pass the southern meridian instead of the northern.

The slowness of the pole-star's motion, though it renders its transit uncertain to a few seconds, cannot materially affect the accuracy of this method, as an error of ten seconds of time in the estimation of its passage, which is certainly more than can be committed, would not cause an error of a third of a second of time in the passage of stars near the equator.

Example of the Computation with the Numbers given above.

	Star A.				Pole-Star.	
Sin.	IK	8 241855		Sin.	IK	8.241855
Sin.	ZA	9.998844		Sin.	Z 3	9.810316
Sin.	AC	8.240199		Sin.	3 B	8.052171
Sin.	PA —	9.860562		Sin.	P 3 —	8.484848
Sin.	APC	8.379637		Sin.	3 PB	9.567328
	APC	1° 22′ 20″			3 PB	21° 40′ 10″
In time		5m 29⅓s		In time		1h 26m 40⅔s

The error of a degree, therefore, in the position of the transit telescope at the horizon, causes the star A to pass through it 5h. 29⅓s. in time later than it ought, whereas, the same error causes the transit of the pole-star to be 1h. 26m. 40⅔s. later than it ought; and the difference between these two times, viz. 1h. 21m. 11⅓s. will be the difference of the observed time of their transits, owing to the error of the position of the transit telescope, their real right ascensions being supposed the same.

VI.

Observations on the Variation, and on the Dip of the Magnetic Needle, made at the Apartments of the Royal Society, between the Years 1786 *and* 1805 *inclusive. By Mr.* GEORGE GILPIN.*

Of the Variation Compass.

Variation compass of the Royal Society.

THE variation compass, used in making the following observations, is the same instrument used in former observations of the variation, and published by the Society in several volumes of their Transactions; and as a particular and accurate description of its construction was given by Henry Cavendish, Esq. F. R. S. in the LXVIth volume, it will not be necessary to say any thing here on the subject. But these observations being the first that have been communicated since the compass was put up in the Society's apartments in Somerset Place, it may not be amiss to point out its situation in the house at the time of observation, and the method pursued to attain such allowances as were proper to be made in deducing the results here given.

Its situation, and the position of the azimuth mark described.

1. The compass in the house, at the time of observation, was placed in the middle window, on the south side of the Society's meeting-room, upon a strong mahogany board 1¼ inch thick. Against the opposite building the dial-plate of a watch is fixed, making an angle with the true meridian of 31° 8',8 to the eastward, as a mark to which the telescope of the compass was adjusted. To obtain the angle that this mark made with the true meridian, I fixed a transit-instrument on the mahogany board above mentioned, precisely in the same place where the compass had been placed, and having adjusted its telescope to the same mark, the transits of the sun and stars over a vertical circle passing through the zenith and this mark, were observed; and the angle contained be-

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* Philosophical Transactions, 1806.

tween the said mark and the true meridian, was found by computation to be 31° 8',3 as above.

Method of ascertaining the true line of magnetic direction, by inverting the needle

2. For the purpose of ascertaining what error there might be, from a want of parallelism between the line joining the indices and the magnetism of the needle, and thereby to determine whether, in the usual method of observing, the indices shew the true angle which the direction of magnetism makes with the first division or zero, a great many observations were made on both ends of the needle, and with both sides of the needle uppermost (the cap of the needle being made to fit on readily on either face for this purpose), viz. north end and south end in its upright position, and north end and south end with the needle inverted, and the mean of the four giving the angle greater by 2', than that shewn by the north end in the upright position of the needle (which was the end always used in these observations), two minutes have been added to all the observations read from the instrument, as the correction for this error to angles on the east side of zero, and subtracted from angles on the west side, to obtain the true angle; which error to angles on the west side, however, only occurred when the instrument was taken out of doors, to determine the effect of the iron work of the building.

—and of ascertaining the influence of iron in the building;

3. The variation compass being placed in the house for observation, could not be supposed to be entirely out of the influence or iron; I was therefore desirous to ascertain how far that influence might extend: for the determination of which, the following method was adopted.

Having caused to be sunk into the earth, to some depth, a strong post, in the wood-yard of Somerset-House, at a considerable distance from the influence of any iron, on which the compass might be placed, and from which station there was a convenient mark, at a proper distance, to which its telescope might be adjusted: I took the compass there at those times of the day when the needle was stationary, viz. morning and afternoon. Before the compass was carried out of doors, observations were made in the room; then it was taken out of doors to the above-mentioned station, for observation there; and the observations were again repeated, after the compass had been restored to its situation in the room; so that had any alteration taken place in the interval, such alteration would have been detected; but during the whole series, no material difference occurred between the observations made in the house before, and after those taken in the yard.

—by observations made out of doors, and compared with those within.

The observations therefore made in the yard, compared with those taken in the house both before and after those taken out of it, formed the comparison for obtaining the error, or the effect of the iron-work of the room on the needle in the house, and there is reason to believe that considerable accuracy has been obtained. They are as follow:

By a mean of 20 sets, or 200 observations taken with the compass in the yard, compared with twice that number taken in the house, before and after those taken in the yard, the variation observed in the house was found to be greater than that observed in the yard by 5′,4. The mean of nine sets of observations taken in the morning giving for the error 5′,5; and the mean of eleven taken in the afternoon giving for the error 5′,3. The variation in those tables have therefore been lessened by the above-mentioned quantity 5′,4, as the error for the effect of the iron-work of the room on the needle in the house.

Coroborating fact as to accuracy.

I must not omit to mention, that of these 20 sets of observations mentioned above, nine only were made with the compass in the same situation, and eleven in that of a different one; for, after nine sets had been taken, a pile of boards was put up between the compass and the mark to which it had been adjusted, which made it necessary to remove the post on which the compass had been placed, a few feet to the westward of its former situation, to clear it from the said pile of boards; and eleven sets of observations were made from this new station, with the compass adjusted to the same mark it had been adjusted to before, and the angles that this mark made with the true meridian from each of these stations, were ascertained by placing a transit-instrument precisely where the compass had been placed, and observing the transits of the sun and stars, in the same manner as has been described in finding the angle of the mark that the compass was adjusted to in the house. And it is conceived that this accidental circumstance adds some weight to the accuracy with which these operations were performed, as the error from the two results of nine, and eleven, does not differ so much as 0′,5 from each other.

Dipping Needle.

The dipping-needle with which the observations in this communication were made, being the same instrument used in former observations of the dip, and it having also been described by Mr. Cavendish in the paper before alluded to, it will not be necessary to say any thing of its construction here. Its situation in the house was in the eastern window in the meeting-room, next the door.

The dipping-needle.

As the observations made with the dipping-needle were not affected by any other source of error than that of the iron-work of the room, in order to ascertain the quantity of error, the instrument was taken out of doors at two different times, after an interval of ten years, differently situated each time, and the observations made at both these times out of doors, compared with the observations made in the room, giving for the error 20′ more than the dip was found to be in the room, and both agreeing to one minute; that quantity has been added to all the observations made with the dipping-needle in the room for its error, as affected by the iron-work of the room.

Although a valuable paper on the diurnal variation of the horizontal magnetic needle, by the late Mr. John Canton, F.R.S. was published in the first part of the LIst volume of the Phil. Trans. for the year 1759, containing a great number of observations made at different and irregular times of the day throughout the year, yet, it appeared to me, that if the variation were to be observed at short but stated intervals of the day for one year, the results would perhaps not only prove more satisfactory in determining the times of the needle becoming stationary, but would show its progressive and regressive motions better than if observed at irregular intervals. To effect which, I imposed this laborious task upon myself for the space of sixteen months.

Canton's observations on diurnal variation. Those of the author were carried through sixteen months.

The observations contained in Table I. in sixteen pages, viz. from September 1786, to December 1787, both inclusive, are the results made at many but stated times of the day, and so disposed, that the progress, or regress, of the variation, may be readily seen by mere inspection.*

Tabulated results.

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* For these, on account of their length, reference must be had to the Transactions.

Table II, contains the mean monthly variation for the above-mentioned times of the day contained in Table I.

Table III, contains, besides the mean monthly true variation, and mean monthly diurnal alteration of variation, for the sixteen above-mentioned months, the mean monthly true variation, and diurnal alteration of variation for many months in the year, between the years 1786 and 1805 inclusive.

The numbers put down in Table I, are each of them a mean of five observations, and often more.

Those in Table II. depend on Table I.

As the observations from which the true variation has been given in Table III, between the years 1788 and 1805, were too numerous to be all inserted, it has been thought sufficient to give the mean monthly true variation, and mean monthly diurnal alteration of variation only; and they were determined from a mean of the observations made at those times of the day when the variation was considered least, and greatest; which variations for each month, may generally be considered as a mean of 600 observations.

Remarks on the changes of the variation, &c.

From the observations made by the late Dr. Heberden and others, about the year 1775, the variation was found to increase annually nearly 10′, since that time to the present, its rate of increase has been considered as gradually diminishing*, and for

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* An exception to the progressive increase appears between the years 1790 and 1791, as the observations between these two years make it to decrease 2′ or 3′, and subsequent observations to increase again. To what this should be attributed, I am at a loss to account, unless it arose from the alteration which took place in the iron-work of the room in December 1790, four strong iron braces having been applied to the girders in the floor of the great room of the Royal Academy (which is over the Society's meeting-room), in consequence of a cracking noise made from the great pressure of a number of persons in the room during the time that Sir Joshua Reynolds was delivering a lecture: these braces were applied two on each side of, and equidistant from, the compass, the nearest about 18 feet from it. It may be proper to mention, however, that having been favoured with the variation observed both by Mr. Cavendish and Dr. Heberden, in the above-mentioned years, the alteration of the variation was by the former nearly the same as in my own, but by those of the latter, greater in both cases.

An alteration took place between the observations made with the for the last three or four years, the alteration has been so very small, as to make it somewhat doubtful whether it may not be considered stationary, but I would not from so short a period conclude that it really is so.

Remarks on the changes of the variation, &c.

From the observations of sixteen months, viz. from September, 1786, to December, 1787, both inclusive, the variation may be considered as generally stationary at or about 7 or 8 o'clock in the morning, when it is least; and about 1 or 2 o'clock in the afternoon, when it is greatest; and therefore it has been the practice in determining the true variation, put down in the tables, to take a mean of the two morning, and the two afternoon observations, made at those times, for the true variation.

In March, 1787. The mean monthly diurnal alteration of variation was found to be 15',0; in June 19'6; in July 19',6; in September 14',8; and in December 7',6. But on a mean of 12 years observations, from the year 1793 to 1805, the diurnal alteration of variation in March was only 8',5; in June 11',2; in July 10',6; in September 8',7; and in December 3',7.

Table of state of the variation for 12 years, quarterly.

Table IV. contains the differences for 12 years, viz. from 1793 to 1805, between the observations of the variation made in the months of March, June, September, and December, or at the times of the vernal and autumnal equinoxes, and summer and winter solstices; by a mean of these 12 years, the variation appears to increase or go westward, from the winter solstice to the vernal equinox 0',80; diminishes or goes eastward from the vernal equinox, to the summer solstice 1',43; increases again from the summer solstice to the autumnal equinox 2',43; and continues nearly the same, only decreasing 0'14, from the said equinox to the winter solstice.

These differences at the time of the equinoxes and solstices have been noticed by M. Cassini, in his observations made at

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dipping-needle in the same years. All the iron braces were on the north-west side of the needle, and the nearest about 18 feet from it.

The allowances made to the observations of the variation, and also of the dip, for the effect of the iron-work of the room, were both ascertained after the above-mentioned alteration in the iron-work took place; but they have, notwithstanding, been applied to the observations made before, as well as since that time.

Remarks, &c.

at the Royal Observatory at Paris, between the years 1783 and 1788, but the effect was considerably greater in his observations, than in those mentioned above; his results however were, in my opinion, drawn from too few observations, being from only 8 days observations about the times of the equinoxes and solstices, which differ considerably among themselves; and experience teaches us, that magnetical observations, made for a period so limited, are not sufficient for minute purposes: I have, therefore, in the results here given, taken the mean of the observations made during the whole month in which the equinoxes and solstices fall, which appear to me likely to furnish results more satisfactory; and all the foregoing observations are to be considered as the results or mean of a great many, by way of arriving at greater accuracy than could be obtained without; this, however, was found to be more necessary at some times than at others; sometimes the needle would be extremely consistent with itself, so as to return exactly to the same point, however often it might have been drawn aside; at other times it varied 2 or 3′, sometimes 8, 10′, or even more: this uncertainty in the needle arises principally, I believe, from changes in the atmosphere, for a change of wind, from any quarter to another, almost always produced a change in the needle from steady to unsteady, and *vice versâ*, but it was generally more unsteady with an easterly wind than when it blew from any other quarter, and most steady when the wind was south or south-westerly. An Aurora Borealis always produced considerable agitation of the needle.

Unsteadiness of the needle.

Annual increase of the variation.

It has been mentioned in this Paper, that the annual increase of variation was found about the year 1775 to be nearly 10′; and was considered at that time to be gradually diminishing; but it is remarkable that this rate of increase appears, from the annexed Table, to be nearly the same at which it has been found to move between all the different periods in the said Table, from 1580 to 1787, a period of more than 200 years, excepting between the years 1692 and 1723: the observations of Halley in 1692, and Mr. Graham in 1723, make the annual increase 16′: to what this difference could be owing I am at a loss to account: on referring to observations made at Paris for those two years, the annual increase is 14′; subsequent observations made by Mr. Graham in 1748, make the annual increase between this year and 1723 only 8',1 nearly what its rate had been found before this great difference occurred; and from the variation of Mr. Graham in 1748, and the variation observed by Dr. Heberden in 1773, the annual increase is 8',4; the variation in 1773, compared with the variation observed by myself in 1787, give for the annual rate of increase 9',3; but between 1787 and 1795, the annual increase was only 4',7; between 1795 and 1102, 1',2; and between 1802 and 1809, only 0',7.

The mean rate of annual increase for the above mentioned period of 207 years, viz. from 1580 to 1787, is 10'.

Changes in the variation for 200 years.

As there appears something curious in the rate at which the variation has been moving, from observations made at London, for a period of more than 200 years, the annual increase of which during that time continued nearly the same; but in a subsequent period of 18 years only, the decrease of that annual increase became so rapid, that the annual increase in the latter part of it does not amount to quite one minute, I shall subjoin the following Table, by way of elucidating what is here mentioned.

Table of variation for 225 years at London.

By whom the Variation was observed	Year.	Variation. Annual	Increase.
		° ′	′
Mr. Burrows*	1580	11 15 E.	
Mr. Gunter	1622	6 0	+ 7,5
Mr. Gellibrand	1634	4 6	9,6
Mr. Bond†	1657	0 0	10,6
Mr. Gellibrand‡	1665	1 22W	10,2
Dr. Halley§	1672	2 30	9,7
———	1692	6 0	10,5
Mr. Graham‖	1723	14 17	16,0
———	1748	17 40	8,1
Dr. Heberden¶	1773	21 9	8,4
Mr. Gilpin	1787	23 19	9,3
———	1795	23 57	4,7
———	1802	24 6	1,2
———	1805	24 8	0,7

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* The observations of Burrows, Gunter, and Gellibrand, in 1634, are taken from Seller's Practical Navigation, 1676. Burrow's observations are said to be the oldest and best in the world; longitude and latitude found by dipping-needle, p. xvi. Gellibrand is said to be the first person who ascertained the variation of the variation, about the year 1695, Phil. Trans. No. 276—278; but if this is the date of the observations by which it was determined, the observations of Gunter, in 1662, show him to have a prior claim; Bond, in his Longitude Found, p. 5 and 6, says, that the variation was first found to decrease by Mr. John Mair, secondly by Mr. Edmund Gunter, thirdly by Mr. Henry Gellibrand, and by himself, in 1640.

† Longitude Found, p. 3.

‡ Ibid. p. 13; and Longitude and Latitude found by Dipping-Needle, p. 6.

§ Phil. Trans. No. 195, p. 565.

‖ Ibid. No. 383, p. 107; and No. 488, p. 279.

¶ Obligingly communicated by his son, the present Dr. Heberden.

Dip of the needle from 1786 to 1807. No diurnal change.

Table V. contains the dip of the magnetic needle from the years 1786 to 1805. For the first sixteen months, viz. from September, 1786, to December, 1787, both inclusive, the dip was observed as frequently as the variation, *but as there does not appear to be any diurnal alteration in the dip*, to make it at all interesting to communicate so many observations as were made, the mean therefore for each month has been thought sufficient for insertion.

To explain the foregoing Table it must be observed, that each of the numbers in the four first columns of the above Table, are each of them the mean of several means, as expressed in the line against those numbers; and as each of those means, are again the mean of five observations at least, each of the numbers in the first line, said to be the mean of nine means, is therefore a mean of forty-five observations; and so of all the rest.

The numbers in the fifth column, entitled true dip, are the means of the numbers contained in the four preceding columns in the same line with it.

The dip seems to have formerly encreased; but for the last century has inercased.

The dipping needles used by Norman, the inventor of the dipping needle, who observed the dip at London in the year 1576 to be * 71° 50′; and of Mr. Bond, who observed it in 1676 to be † 73° 47′; not being so much to be depended upon as the needles that have been in use for near a century past, render the progressive increase of the dip from Norman's time, to the time of its maximum, somewhat doubtful. But Mr. Whiston, whose needle there is reason to believe was more to be relied upon, in the year 1720 determined the dip to be ‡ 75° 10′; this, when compared with many, and very accurate observations made by Mr. Cavendish with several needles, in the year || 1775, who found it to be 72° 30′, makes the decrease in this period, of 55 years on a mean, 2′ 9 *per annum*. And from a comparison of my own observations of the dip in 1805, which was 70° 21′, with the above of Mr. Cavendish in 1775, its annual decrease, on a mean, appears to have been 4′, 3; and its progressive annual decrease, on a mean, in the above mentioned period of 30 years, 1′, 4.

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* New Attractive, c. 4. † Longitude found.

‡ Longitude and latitude found by dipping needle, p. 7—94.

|| Phil. Trans. Vol. LXVI, p. 400.

It is much to be regretted that observations of variations have not been oftener made.

I cannot conclude this Paper without expressing my regret, that so little avail should have been made of the numerous opportunities which have been afforded to travellors and others, in the last century, for making accurate observations, with proper instruments, at land, on the variation in different parts of the world. Such observations would probably have afforded some curious and useful facts, which would have materially assisted in forming a theory much more certain than what we at present possess; the present received opinion of the cause of the diurnal alteration of variation would be confirmed or invalidated; its quantity of effect in different places, a most desirable acquisition, would be ascertained; and we should be put in possession of more valuable and correct information on the variation than can be derived from observations made with the common azimuth compass, even at land, owing to its imperfect construction. The variation thus accurately obtained at any one period, compared with the variation correctly ascertained at a subsequent period, would give a rate of alteration of the variation which could be relied on.

Obcrvations and researches of Halley.

The celebrated Halley thought the variation of so much importance, that he made two voyages for the purpose of making observations on the variation, to confirm his theory advanced in 1613, and soon after he published his variation chart. Since his time no better theory than he left has been obtained, although it must be confessed that many observations have been made at sea by voyagers; but these observations, made generally to answer the purpose of the observer at the time only, are therefore seldom preserved; for, unless made by authority, which rarely happens, they do not often meet the public eye; and it must be from observations made with care, and with good instruments, carefully registered, and properly arranged, that any real advantage can be derived. It is hoped therefore, that, in future, attention to this subject will not be thought beneath those who may have it in their power essentially to promote an undertaking so interesting to the philosopher, and so valuable and useful to the maritime world.

TABLE II.—*Mean monthly Variation of the magnetic Needle.*

1786	6 A.M.	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Sept.	23 —	23 7,9	23 10,1	23 14,5	23 22,2	23 23,7	23 23,9	23 19,0	23 15,3	23 13,5	23 12,4	23 —
Oct.	—	10,1	11,3	15,2	24,4	26,1	26,1	21,1	17,7	15,6	14,5	13,8
Nov.	—	12,2	12,5	15,3	21,6	22,5	22,0	20,3	17,6	15,9	15,1	14,7
Dec.	—	—	14,5	16,1	20,6	22,0	22,2	20,0	17,4	15,8	15,0	15,0
1787												
Jan.	—	14,0	14,2	17,1	22,5	24,1	24,5	21,8	18,4	15,6	14,5	14,8
Feb.	—	14,2	15,1	17,1	23,3	24,8	25,1	23,7	18,8	15,3	15,8	12,8
Mar.	—	12,8	12,8	15,3	26,5	27,7	27,8	18,4	19,0	15,9	15,5	15,7
April	9,7	9,9	9,7	13,9	23,6	27,0	27,4	22,6	17,8	15,7	15,7	15,6
May	7,6	7,5	7,4	13,5	25,2	26,6	26,2	21,0	17,7	17,1	16,8	17,0
June	8,4	8,2	8,8	16,0	26,6	28,1	28,1	22,6	18,7	17,9	17,8	17,7
July	9,5	9,6	10,3	17,8	27,6	29,3	29,4	23,2	19,4	18,9	19,3	19,1
Aug.	11,9	12,0	12,8	19,7	30,3	31,7	31,5	25,6	19,3	18,7	18,9	18,8
Sept.	15,0	15,1	15,3	20,2	29,8	30,7	30,5	24,7	20,1	19,1	19,2	19,2
Oct.	—	17,5	17,3	21,1	30,8	31,9	31,5	27,4	21,9	20,8	20,2	19,6
Nov.	—	19,4	19,7	20,6	29,7	31,1	30,2	27,7	22,7	21,4	21,3	21,4
Dec.	—	20,4	21,0	21,8	28,2	29,0	29,0	26,2	22,9	21,9	21,6	—

TABLE III.—*Mean monthly true Variation, and mean monthly diurnal Alteration of Variation of the magnetic Needle.*

	January.		February.		March.		April.		May.		June.	
	True Variation	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.
	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′
1786	—	—	—	—	—	—	—	—	—	—	—	—
1787	23 19,2	10,2	23 19,8	10,4	23 20,3	15,0	23 18,5	17,4	23 17,0	18,9	23 18,3	19,6
1788	23 25,6	8,7	—	—	—	—	—	—	—	—	23 38,3	18,8
1789	—	—	—	—	—	—	—	—	—	—	23 34,2	17,1
1790	23 38,9	8,4	—	—	—	—	—	—	—	—	—	—
1791	23 35,6	6,8	—	—	—	—	23 36,0	15,0	—	—	—	—
1792	23 41,1	5,4	—	—	—	—	—	—	23 41.9	11,5	—	—
1793	23 43,9	4,3	23 48,3	4,6	23 48,8	8,5	23 46,2	11,7	23 47,3	10,[illegible]	23 48.5	12.6
1794	23 54,2	4,5	—	—	—	—	—	—	—	—	—	—
1795	—	—	—	—	23 57,5	9,8	—	—	—	—	23 57,1	9,4
1796	—	—	—	—	24 1,1	7,0	—	—	—	—	23 58,7	9,8
1797	—	—	—	—	24 1,5	7,4	—	—	—	—	24 0,2	11,6
1798	—	—	—	—	24 0,6	7,2	—	—	—	—	23 59,4	11,2
1799	—	—	—	—	24 1,1	7,5	—	—	—	—	24 0,6	10,8
1800	—	—	—	—	24 3,6	6,9	—	—	—	—	24 1,8	10,9
1801	—	—	—	—	24 5,0	8,8	—	—	—	—	24 2,8	10,8
1802	—	—	—	—	24 6,9	9,5	—	—	—	—	24 5,	10,7
1803	—	—	—	—	24 8,0	11,8	—	—	—	—	24 7,0	12,6
1804	—	—	—	—	24 9,4	10,0	—	—	—	—	24 6,0	11,3
1805	—	—	—	—	24 8,7	8,1	—	—	—	—	24 7,8	12,5

TABLE III.—*Mean monthly true Variation, and mean monthly diurnal Alteration of Variation of the magnetic Needle.*

	July		August.		September.		October.		November.		December.	
	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.
	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′
1786	—	—	—	—	23 16,4	14,8	23 18,4	15,3	23 17,3	9,9	23 18,3	7,6
1787	23 19,6	19,0	23 21,9	19,1	23 22,8	15 5	23 21,5	[illegible]	23 25,0	11,1	23 25,8	8,3
1788	23 29,8	16,4	—	—	—	—	23 32,1	14,6	—	—	—	—
1789	—	—	—	—	—	—	—	—	—	—	23 41,2	5,4
1790	23 33,0	15,4	—	—	—	—	—	—	—	—	—	—
1791	23 36,7	15,2	—	—	—	—	—	—	—	—	—	—
1792	—	—	23 13,6	12,7	23 43,9	11,1	23 45,6	8,9	23 45,9	3,7	23 45,3	3,1
1793	23 50,5	12,5	23 48,6	12,[illegible]	23 52 0	9,8	23 52,3	7,0	23 51,9	3,8	23 52,3	3,8
1794	23 51,4	11,2	23 57,2	9,9	23 55,1	8,4	—	—	—	—	—	—
1795	23 5[illegible],1	8,4	—	—	24 0,4	7,6	—	—	—	—	23 59,4	3,6
1796	23 69,2	10,1	—	—	24 0,1	8,3	—	—	—	—	24 1,3	4,9
1797	24 0,3	10,1	—	—	24 1,4	7,6	—	—	—	—	24 1,3	5,0
1798	24 0,0	10,0	—	—	24 1.4	9 4	—	—	—	—	24 1,4	2 7
1799	24 1,8	10,4	—	—	24 2,9	7,8	—	—	—	—	24 2,3	3,4
1800	24 3,0	9.2	—	—	24 3,6	7,7	—	—	—	—	24 3,3	3,1
1801	24 4,1	10,5	—	—	24 3,8	10,1	—	—	—	—	24 5,4	2,5
1802	4 6,0	12 3	—	—	24 8,7	8,9	—	—	—	—	24 6,8	3,8
1803	24 7,9	13,1	—	—	24 10,5	9,5	—	—	—	—	24 10,7	3,0
1804	24 8,4	10,4	—	—	24 8,9	9,3	—	—	—	—	24 9,0	3,7
1805	24 7,8	10,4	—	—	24 10,0	9,3	—	—	—	—	24 9,4	4,6

TABLE IV.—*Differences between the Observations of the Variation of the magnetic Needle, at the times of the Equinoxes and those of the Solstices.*

Years.	March.	June.	September	December.
	′	′	′	′
1793	+ 3,6	— 0,3	+ 4,1	— 0,3
1795	——	— 0,4	+ 3,3	— 1,0
1796	+ 7	— 2,4	+ 1,4	+ 1,2
1797	+ 0,2	— 1,3	+ 1,2	— 0,1
1798	— 0,7	— 1,2	+ 2,0	0,0
1799	— 0,3	— 0,5	+ 2,3	— 0,6
1800	+ 1,3	— 1,8	+ 1,8	— 0,3
1801	+ 1,9	— 2,4	+ 1,0	+ 1,6
1802	+ 1,5	— 1,6	+ 3,4	— 1,9
1803	+ 1,2	— 1,0	+ 3,5	+ 0,2
1804	— 1,3	— 3,4	+ 2,9	+ 0,1
1805	— 0,3	— 0,9	+ 2,2	— 0,6
Mean	+0′.80	—1′.43	+ 2′,43	— 0′,14

VII.

A few Remarks on a Pamphlet entitled "Mr. W. Nicholson's "Attack, in his Philosophical Journal, on Mr. Winsor and "the National Light and Heat Company, with Mr. Winsor's "Defence"—12mo. 56 pages.

Mr. Winsor's pamphlet lately published.

MR. Winsor seems to have overlooked the observations in our Journal for January last, as long as prudence and the inquiries of his visitors would allow him to follow that mode of conduct. After having answered a question of public importance openly and without reserve, in my own name, as every man ought, where the character of another can in any respect be brought in question, I may be allowed to decline all controversy, and leave Mr. Winsor's claims upon me and upon the world, to be settled from the facts as they stand. I think it would be extremely easy to shew the numerous, and, in some instances voluntary, errors and misrepresentations with which his pamphlet is vitiated; but I am called upon by no duty to do this.

Reasons why the Editor notices it.

To my readers, it is, I trust, needless to repeat the truths I have already laid down. I can have no cause of enmity to Mr. Winsor; but I am not indifferent to the question, whether the public shall be deluded, when one of that public asks me to give an opinion upon what every man has a right to examine. It is this motive which leads me at present to notice his pamphlet, and induces me not to dismiss the subject without a few general observations.

Mr. Winsor has not given satisfaction to the public.

It certainly is Mr. Winsor's duty to give all those means of satisfaction in his printed proposals, which are usually tendered when undertakings of credit are offered for public support. The following remarks will shew that he has not done this:

His pretended committee is unknown,

1. He asserts, that a public committee was appointed to verify his discovery. He ought to have said who appointed them, where are their minutes, and *what are their names*.

—and also the proprietors who propose the company.

2. He pretends that his patent is vested in four respectable gentlemen, as co-proprietors, who propose to establish a company. *He ought to have published their names.*

Either Mr. Winsor holds the first £20,000, or the holders are concealed.

3. On behalf of these four concealed gentlemen and himself, he asks for subscriptions; of which the proposed deposits amount to one hundred thousand pounds, by twenty thousand subscribers; and there is to be no meeting of the intended company till one fourth part of this sum (twenty-five thousand pounds) has been deposited. Mr. Winsor either has an uncontrouled power to draw this £20,000 from the banking-houses, or he has not. If he has not, the sum is in trust either with the four co-proprietors, or with other nominees, or with the bankers. In any or either case, the trust ought to have been declared and published; and the trustees themselves, by name, would in fact stand pledged for the honour and credit of the whole project.

Mr. Winsor ought to establish his patent, by offering a plain question to counsel,

4. If Mr. Winsor, in his pamphlet, instead of running into a long and faulty dissertation on the law of patents, had laid my remarks (since the dignity he asserts has not forbad him to notice them) before counsel; or if he had asked the simple question of any eminent legal man—" Whether the granting of

" licences, to exercise any part of a patent privilege, to a " number exceeding five, *limited by covenant*, be not such a " sharing of the monopoly as will render the patent void?"— He would have shown at once how far I may have presumed to speak positively where it became me to doubt; and he might have set the minds of his subscribers at rest upon that point. I have asserted, that if he has disposed of such licences, which he very properly calls *sharing the privileges*, to a thousand persons out of a limited twenty thousand, as he asserts, he has annulled his patent. He will not find a lawyer who will maintain the contrary.

I am persuaded that the magnitude, as well as the philosophical nature of the subject, and the interest which so large a part of the manufacturers of the British empire have in patent rights, will render the preceding observations of sufficient importance to require no apology.

VIII.

Account of the Small Whales in the Seas near the Shetland Isles. By PATRICK NEILL, *A. M. Secretary to the Natural History Society at Edinburgh.**

Shoal of small whales.

BY a letter from a gentleman at Uvea Sound, Unst, I was informed, that, " on the 21st February, 1805, no fewer than 190 small whales, from six to twenty feet long, were forced ashore at Uvea Sound; and on the 19th March thereafter, 120 more at the same spot, in all, 310. In this second shoal there were about 500, but very many escaped." To a series of queries addressed to the same gentleman, I received in substance the following answers. " They measured from six to twenty-four feet in length: the small ones appeared to be the young of the others. They had two long and narrow pectoral fins, from between four and five feet to even nine feet long. They remained at the surface of the water ten or fifteen minutes

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* From his Tour to the islands of Orkney and Shetland.

Account of the small whales which frequent the north coast of Scotland, &c.

nutes, just as the boats were near or distant. They had one small fin on the back. The people called them *bottle-noses*, and *common black whales*, but most generally *ca'ing whales*. They had a row of teeth, $1\frac{1}{2}$ inches long, in both jaws, about two dozen in number in each jaw. The upper jaw was rather the widest. They had no whalebone in the mouth, and had only one blow-hole, situated in a small hollow at the back of the head. Most of the females w[illegible] either with young or giving suck. Many of the young ones had got no teeth. They had all very fine black skins, as soft and smooth as silk. They appeared to be very inoffensive animals, and shewed much natural affection for each other: when any one first struck the ground, it set up a kind of howling cry, and immediately others crowded to the spot, as for its relief. *Sandy giddocks* (sand-lances) were found in their mouths." From information furnished by another gentleman, I further learned, that " from the tip of the nose to the last vertebra of the back-bone, the generality of the whales measured twenty feet: that the head was short and round, resembling in shape the head of a seal; and the upper jaw projected three or four inches over the lower."—Numbers of the females (this gentleman adds) were suckling their young when driven ashore; and while they continued alive, the milk was seen to issue from their nipples: of these they had only two, resembling the teats of a cow, but larger."

This kind of whale sometimes appears, in large herds, off the Orkney, and especially the Shetland islands. Being of a gregarious disposition, the main body of the drove follows the leading whales, as a flock of sheep follows the wedders. Hence the name of *ca'ing whales*, bestowed on them by the natives, who well know that if they are able to guide the leaders into a bay, they are sure of likewise entangling multitudes of their followers. Though the above description proves that they belong to the genus Delphinus. and are nearly allied to the Delphinus Orca or Grampus, they appear to me to differ in several respects from that, or any of the other species described by naturalists, so much at least, as to deserve the attention of gentlemen who may hereafter enjoy opportunities of accurate observation. I shall briefly enumerate the points of dissimilarity.

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The grampus has the snout "spreading upwards," according to Shaw[*]; "waved upwards." according to Stewart[†]; "*sursùm repando*," as Linnæus expresses it. But this character was not to be found in the *ca'ing whales*, in which the nose was neither spread nor turned up at the end, but rounded and dropping. But I must remark, that La Cepède (the able continuator of Buffon's "Histoire Naturelle," and whose general accuracy is great) takes [illegible] notice whatever of the "waving or spreading upwards," the "sursùm repando," mentioned by preceding authors.

In the grampus, according to Shaw, "the lower jaw is much wider than the upper," in the ca'ing whale: however, we find that "the upper jaw is the widest."

The grampus is said, in books, to have thirty teeth in each jaw: the Uvea-Sound whales had only twenty-four in each jaw. But La Cepède remarks, that the number of visible teeth varies with the age of the animal.

In Dr. Shaw's figure of the grampus (which, I must confess, is inferior in accuracy to that of La Cepède), the pectoral fins are short and round; according to La Cepède, they are "*larges et presque ovales*[‡]." In the ca'ing whale they are said to be long and narrow,"—thus bearing more resemblance to those of the Delphinus gladiator (to be afterwards spoken of).

"The back fin," says Dr. Shaw, "measures six feet in height." In the largest of the Uvea whales it did not exceed two feet. La Cepède does not make it so long as Shaw.

The eye of the ca'ing whale, I am informed, was placed higher in the head than in Shaw's figure; and the spiracle, as we have seen, was "situated in a small hollow at the back of the head," and behind the eye: no such hollow is delineated in Dr. Shaw's plate; but this is probably an oversight, as it is distinctly depicted in La Cepède's representation of the same animal.

The Uvea whales had not the white spot on each shoulder, near the eye, described as appearing in the grampus, and figured

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[*] "General Zoology," *in loco.*

[†] "Elements of Natural History," 2 vols. 8vo.

[‡] "Histoire Naturelle des Cétacées, par le citoyen La Cepède," p. 301, 4to. Paris, l'an xii.

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figured by Shaw. But La Cepède only says, "On voit *souvent* derrière l'œil une grande tache blanche.[*]"

The neck, breast, and belly were not, I am told, white, as in the grampus, nor was there a defined line between the dark and light parts. Some of the ca'ing whales were, according to my information, quite black; others, especially females, had only a little grey on the belly.

The grampus, we are told[†], "seldom remains a moment above water:" the Uvea whales, however, as formerly observed, "remained ten or fifteen minutes at the surface, just as the boats were near or distant."

The grampus is stated by Dr. Shaw to be a "very ferocious animal, attacking seals and porpusses:" it has long been considered as the formidable *sea-monster* spoken of by the ancients[‡]: but the *ca'ing whale* appeared to be a very inoffensive animal, and the common sand-lance was observed to be its food.

Under the name of *grampus*, a similar animal, called by La Cepède, *le Dauphin gladiateur*, has generally been confounded. The dorsal fin, however, stands much higher than in the grampus, and nearer to the head. The pectoral fin is long and narrow like an oar. It is this species, and not the common grampus, that attacks whales, fastening around them like so many bull-dogs, and making them bellow with pain: hence sailors call it the *killer*. One of this species was, in 1793, taken in the Thames; a drawing and description of which appears

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[*] "Histoire des Cétacées," &c p. 300.

[†] Bingley's "Natural Biography," vol. ii, p. 152.

[‡] The *small-eyed cachalot* (Physeter microps) must certainly be a much more terrible-looking animal. Its head is very large, forming indeed nearly one half of the whole body, which is from 40 to 60 feet long. It is known to be very ferocious, having been seen to attack and tear to pieces the huge Greenland whale. It is not without reason, therefore, that La Cepède rather considers this animal as the *sea-monster* of the ancient mythologists—from the devouring jaws of which Perseus delivered the fair candidate for the prize of beauty (Andromeda), and the horrific aspect of which struck terror into the fiery steeds of Hippolytus. It was a cachalot of this kind that was, in the end of the year 1769, stranded at Cramond, near Edinburgh, and which attracted many thousands of spectators from that city.—Stark's Picture of Edinburgh, p. 465.

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pears to have been sent by Sir Joseph Banks to La Cepède, who has figured it in his "Histoire des Cétacées."

The small whales in question, of whatever species they be, afford a great deal of blubber; and it appears surprising that the value of the oil does not induce some of the Shetland and Orkney gentlemen or some of the few substantial tenants, to prepare and keep in readiness an ample store of harpoons, ropes, whale-lances, blubber-knives, and other implements, so as to enable their dependants to avail themselves, more completely than is at present possible, of the occasional visits of those cetaceous inhabitants of the northern seas. Harpoons and lines are indispensably necessary. The best harpoons, I believe, may be commissioned from Prestonpans, at the rate of 7s. 6d each. A single line for each harpoon would suffice, and that line needs not be of the thickness required for Greenland whales: the Greenland whale-lines cost 5l. but a line sufficient for the small whales might be had for 2l. sterling. Each boat might carry six harpoons and lines, provided only care were taken to keep the lines clear of each other. Each man should be furnished with a *lance, i. e.* a kind of spear with a wooden handle six feet long, costing 5s. each. Blubber-knives may be had at 2s. 6d. each. The hooked instrument called *tomahawk* or *pickihawk*, is also very useful for laying hold of the blubber, and keeping it on the stretch till it be cut. If the blubber is to be barrelled, it should be allowed to lie exposed to the air for a day or two, till incipient putrefaction be perceived; for the swelling that accompanies the commencement of that process would infallibly burst the barrels It is scarce necessary to add, that a large caldron would be found very useful for boiling down the blubber.

The exertions of the Shetland tenants, with respect to such droves of small whales, must certainly be much cramped by the usage of the country, which I have now to relate, and which appears to me equally destitute of foundation in law and in equity. I shall state the usage in the words of Mr. Giffard of Busta, which are certainly above all exception: "As soon as the whales are got ashore (*i. e.* by the exertions of the people, who,

Account of the small whales which frequent the north coast of Scotland, &c.

who, surrounding them with boats, embay them, and force them ashore), the bailie of the parish is advertised, who comes to the place, and takes care that none of them are embezzled; and he acquaints the Admiral thereof, who forthwith goes there, and holds a court, where the fiscal presents a petition, reciting the number of whales, &c. that the judge may give judgment thereupon, according to law and the country practice. Whereupon the Admiral ordains the whales driven ashore to be divided in three equal parts; one to belong to himself; one to the salvers; and the third to the proprietor of the ground on which the whales are driven ashore*." It is added, that the minister of the parish demands tithes of them, and that the bailie of the parish claims the head as a perquisite. Mr. Giffard fortunately informs us, that the "biggest" of the whales of which he is speaking, "are from eighteen to twenty feet long."

Let us now examine how the law stands on this subject. "By the *leges forestarum*, § 17 (says Mr. Erskine), all *great* whales belong to the King, and all such smaller whales as may not be drawn from the water to the nearest part of the land on a wain with six oxen. But no whales have, for at least half a century past, been claimed, either by the King, or by the Admiral his donatory, but such as were of a size considerably larger than there described."

IX.

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* Account of Zealand, by Thomas Giffard of Busta, 1733, in *Bibliotheca Britannica topographica*, No. 38.

† Institute, b. ii, tit. 1, § 10.

IX.

Method of preparing Pannels for Painters. By Mr. S. GRANDI*.

Composition for grounding pannels. It is made of calcined bones and wheat flour.

TAKE the bones of sheep's trotters, break them grossly, and boil them in water until cleared from their grease, then put them into a crucible, calcine them, and afterwards grind them to powder. Take some wheaten flour, put it in a pan over a slow fire until it is dry, then make it into a thin paste, add an equal quantity of the powdered bone-ash, and grind the whole mass well together: this mixture forms the ground for the pannel.

Application.

The pannel having been previously pumiced, some of the mixture above-mentioned is rubbed well thereon with a pumice-stone, to incorporate it with the pannel. Another coat of the composition is then applied with a brush upon the pannel, and suffered to dry, and the surface afterwards rubbed over with sand-paper.

A thin coat of the composition is then applied with a brush, and if a coloured ground is wanted, one or two coats of the colour is added, so as to complete the absorbent ground.

When it is necessary to paint upon a pannel thus prepared, it must be rubbed over with a coat of raw linseed or poppy-oil, as drying oil would destroy the absorbent quality of the ground; and the painter's colours should be mixed up with the purified oil hereafter mentioned.

Canvas grounds.

Canvass grounds are prepared, by giving them a thin coat of the composition, afterwards drying and pumicing them, then giving them a second coat, and, lastly, a coat of colouring matter along with the composition.

The grounds thus prepared do not crack; they may be painted upon a very short time after being laid, and from their absorbent quality, allow the business to be proceeded upon with greater facility and better effect than with those prepared in the usual mode.

Method

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* The processes of Mr. Grandi being founded upon practice, were supported to the Society of Arts, by certificates from our most eminent painters; in consequence of which, and of the exhibition of the Pannels, the Society awarded him the Silver Medal and 20 guineas.

Purifying oil.

Method of purifying Oil for Painting. Make some of the bone-ashes into a paste with a little water, so as to form a mass or ball; put this ball into the fire, and make it red hot; then immerse it for an hour, in a quantity of raw linseed oil, sufficient to cover it: when cold, pour the oil into bottles, add to it a little bone-ash, let it stand to settle, and in a day it will be clear and fit for use.

White colour.

White Colour is made by calcining the bone of sheep's trotters in a clear open fire, till they become a perfect white, which will never change.

Brown.

Brown Colour is made from bones in a similar manner, only calcining them in a crucible instead of an open fire.

Yellow.

Yellow-Colour, or Masticot. Take a piece of soft brick, of a yellowish colour, and burn it in the fire; then take for every pound of brick, a quarter of a pound of flake-white, grind them together and calcine them; afterwards wash the mixture, to separate the sand, and let the finer part gradually dry for use.

Red.

Red-Colour, equal to Indian-Red. Take some of the pyrites, usually found in coal-pits, calcine them, and they will produce a beautiful red

Grey.

Grey Colour is made by calcining together blue-slate and bone-ashes powdered, grinding them together, afterwards washing them, and drying the mixture gradually.

Blue-black.

Blue-Black is made by burning vine-stalks in a close crucible in a slow fire, till a perfect charcoal is made of them, which must be well ground for use.

Crayons.

Crayons are made of bone-ash powder mixed with spermaceti, adding thereto the colouring matters. The proper proportion is, three ounces of spermaceti to one pound of the powder. The spermaceti to be first diffused in a pint of boiling water, then the white bone-ash added, and the whole to be well ground together, with as much of the colouring matter as may be necessary for the shade of colour wanted. They are then to be rolled up in the proper form, and gradually dried upon a board.

White chalk

White Chalk, if required to work soft, is made by adding a quarter of a pound of whitening to one pound of the bone-ash powder will answer alone. The coloured chalks are made by grinding the colouring matter with bone-ashes.

SCIENTIFIC NEWS,

AND OTHER MISCELLANEOUS ARTICLES.

Small portable Fire Engine.

MR. HORBLOWER, of Featherstone Street, City Road, with whose talents the world is well acquainted, has requested me to mention a construction of the fire engine which he has made, which renders it of much utility within the apartments of an house. I have not seen the engine; but he states, that it stands in the compass of fourteen inches square and two feet high, and may be carried from one room to another with ease. He finds, by experiment, that the four sides of a bed-room, all on fire, may be extinguished in the space of a minute, by little more than a pail of water. All that is required is to keep it filled in its proper place, and to work it off every month or six weeks, for the purpose of changing the water and ascertaining that it is in proper working state.

Enquiry respecting Grease Spots.

A Correspondent requests to be informed of a method of discharging grease spots from coloured goods, composed of silk and worsted. He observes, that it frequently happens, in the process of weaving, that the tallow or oil drops on the work from the candles or lamps used by the weavers, and forms spots which render the goods quite unsaleable; and he suggests, that if any of the readers of this Journal should point out a method of discharging them, they would render a considerable service to manufacturers.

I wish it were in my power to point out the remedy here desired from actual experiment; but I must leave the answer to others, and shall only venture to speak in general terms of the means by which spots of grease are usually taken out of piece goods. These methods are reducible to two, namely, absorption and ablution. When an absorbent earth (fullers earth or tobacco pipe-clay for example), is applied wet upon a place which is greased, the oil usually flows into the capillary interstices of the earth, as the water evaporates: and, upon beating or rubbing out the dry earth, the vegetable or animal fibre is left clean. When the oil is solid, at the common temperature, as is the case with tallow or wax, it is found necessary to apply the heat of an iron or common fire cautiously to the place, while the earth is drying. In some description of goods blotting paper, or bran, or raw starch, may be used with advantage. In these manipulations the difficulty of taking out the grease does not seem to be so great as that of avoiding injury to the face of the goods.

The method of taking out grease by ablution is perfectly well known. Water acts upon grease by the medium of soap, or less safely by the interposition of an alkali. The chemical action of these, as well as the probability of mischief from applying water to various descriptions of goods, oppose insurmountable obstacles to their use in many instances. I have not tried how far the solution of pure ammonia might be beneficial in processes of this kind. It promises the advantage of quitting the article by evaporation, after the process is over. There is a method, commonly used for taking grease spots out of silks, which may probably be intitled to farther extension. Alcohol, or spirit of wine, does not act upon grease or fat oil by itself; but when the volatile oil of lemons, called essence of lemons, is dissolved in that fluid, the compound will take out grease spots. The method of applying it is to wet the place, and wipe or rub it while wet with a sponge or cloth. It might be worth trying whether a much cheaper essential or volatile oil than that of lemons might not be used for this purpose. Spirit of turpentine would have an unpleasant smell for a time; but, perhaps, it would not last.

Dr. Clanny, of Durham, has just published an history and analysis of the mineral waters at Butterby, near that town.

I can-

TO CORRESPONDENTS.

IT would be an unpleasant, as well as a difficult task, for me to state the reasons which may at any time require me, as Editor of the Philosophical Journal, to decline inserting some of the papers which may be sent to me. It is obvious that a variety of very proper inducements may offer themselves to govern my conduct in that respect, which it would answer no useful purpose to detail. My Correspondents have accordingly, in almost every instance, received this information in private, where it has been required; but, for the most part, I have been allowed to excuse my discretion without enquiry. Among a few papers which remain with me, and are not intended for insertion, one from H. B. K. has been the subject of enquiry and remonstrance from the writer. As he is unknown, I have no other than the present channel to say, that his Paper will be returned to the bearer of an order, in the same hand writing; and as he complains of a want of justice in its not having been inserted, I must remark, that though his discussions appeared to me to have become too extensive for monthly insertion, I should, nevertheless, have admitted that paper, if I had not thought that the spirit of controversy between himself and Mr. Sylvester was becoming too personal to be interesting to the readers of this Journal.

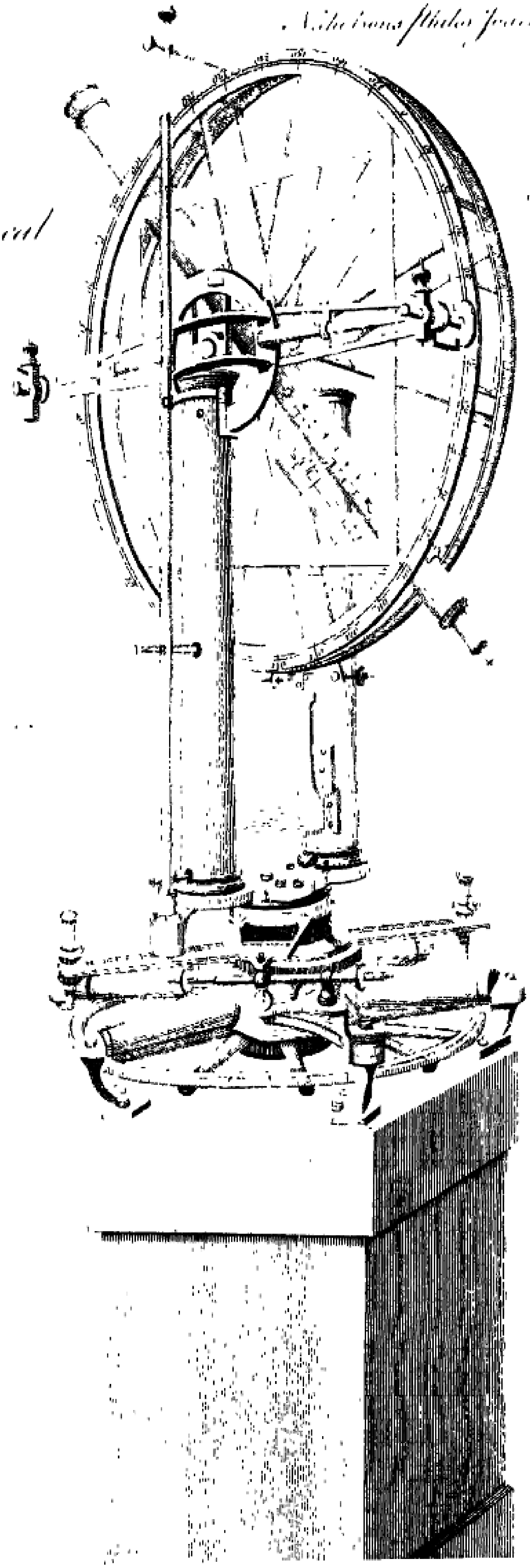
I cannot at present answer the enquiries of R. P. respecting the application of muriatic acid to promote vegetation; but I will satisfy myself whether the alledged facts on that behalf may be entitled to attention.

ERRATA in Dr. BOSTOCK's Paper on PALM OIL.

Page 163, l. 5, *for* alexginons, *read* oleaginous.
l. 9, *for* flashes, *read* flakes.
l. 13, *after* "in" *insert* "the."
Page 164, l. 3, from bottom, *for* resin, *read* resins.
Page 166, l. 17, from bottom, *for* 52 gs. *read* .52 gr.
l. 15, from bottom. *for* 1-8th gr. *read* .8 gr.

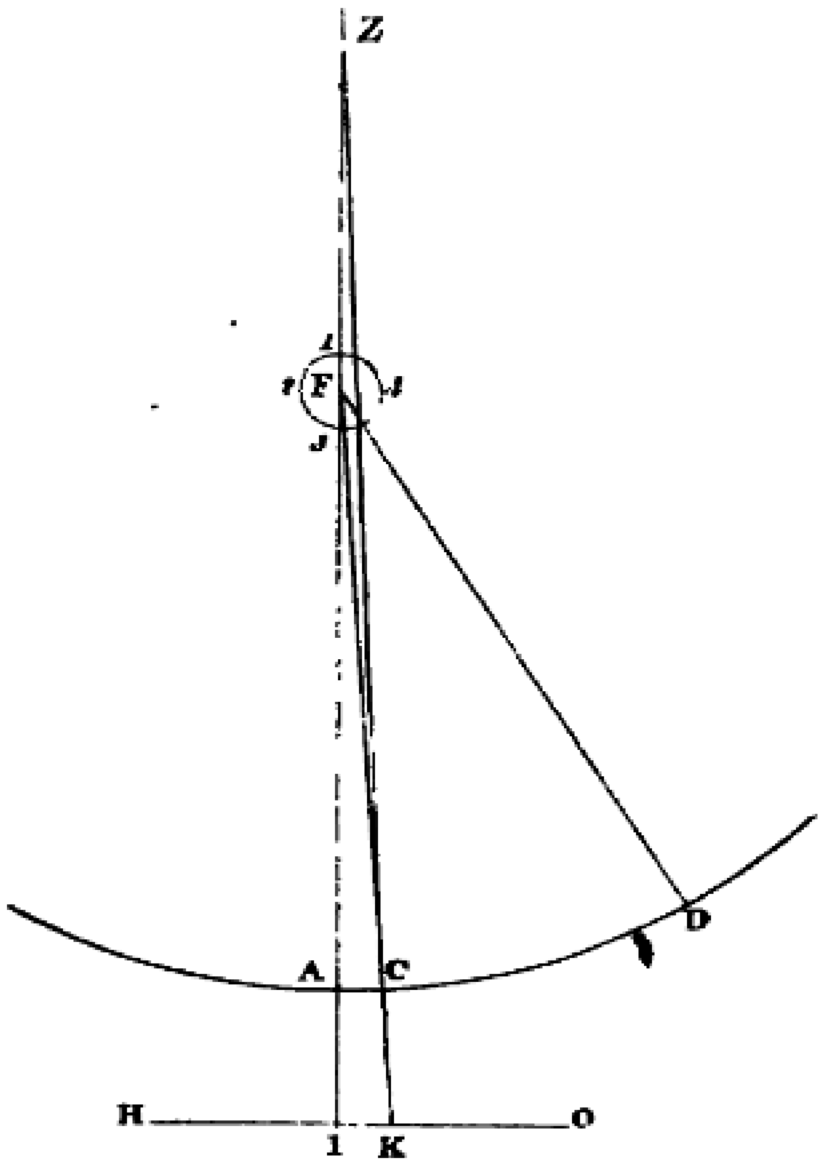
Printed by P. Da Ponte, 15, Poland-Street, Oxford-Street.

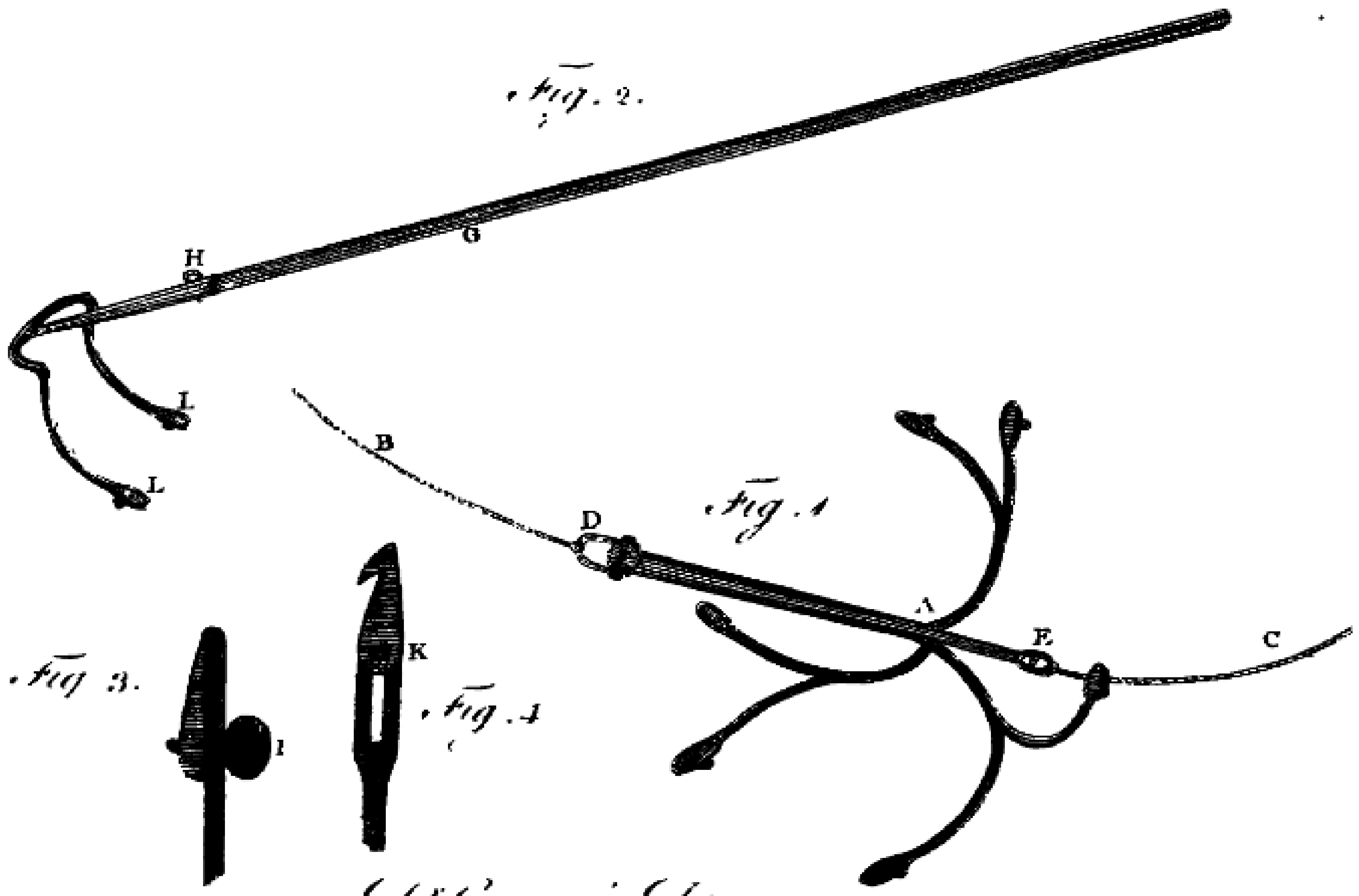
Nicholsons Philos. Journal Vol. XVIII Pl. 7 p. 3[illegible]



Astronomical Circle

Sir H. C. Englefield's method of adjusting the Transit Instrument





Dr. Cogan's Drags

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY

AND

THE ARTS

MAY, 1807.

ARTICLE I.

Description of a New Astrometer for finding the Rising and Setting of the Stars and Planets, and their Position in the Heavens. *By* DAVID BREWSTER, *A. M.*

To Mr. NICHOLSON,

SIR,

Description of an astrometer for determining the apparent situation of the stars, &c

AN astrometer for finding the rising and setting of the stars, was invented about thirty years ago, by M. Jeurat, of the Academy of Sciences at Paris, and is described in the Memoirs of that learned body. The utility of this instrument in abridging the computation of semidiurnal arcs, where great precision is unnecessary, renders it highly interesting to those who are engaged in the study or practice of astronomy, and has induced me to send you the description of a new astrometer, more simple in its construction, and more extensive in its application, than that invented by M. Jeurat.

This astrometer, represented in Plate II. Fig. 1. consists of four divided circumferences. The innermost of these is moveable round the center A, and is divided into twenty-four hours, which are again subdivided into quarters and minutes, when the circle is sufficiently large. The second circumference is composed of four quadrants of declination, divided by means of a table of semidiurnal arcs, adapted to

Description of an astrometer for determining the apparent situation of the stars, &c.

to the latitude of the place. In order to divide these quadrants, move the horary circle, so that 12 o'clock noon may be exactly opposite to the index B: then since the star is the equator, and its declination o, when the semidiurnal arc is VI hours, the zero of the scales of declination will be opposite VI. VI. and as the declination of a star is equal to the colatitude of the place, when its semidiurnal arc is o, or when it just comes to the south point of the horizon, without rising above it, the degree of declination at the other extremity of the quadrant, or opposite XII. XII. will be the same as the colatitude of the place, which in the present case is 39, the latitude of the place being supposed 51° North. The intermediate degrees of declination are then to be laid down from a table of semidiurnal arcs*, by placing the degree of declination opposite to the arc to which it corresponds, thus the 10th degree of south declination must stand opposite V^h 13′ in the afternoon, and VI^h 47′ in the morning, because a declination of ten degrees south gives a semidiurnal arc of V^h 13′. When the scales of declination are thus completed, the instrument is ready for shewing the rising and setting of the stars. For this purpose move the horary circle till the index B points to the time of the star's southing; thus opposite to the stars declination in the scale C, if the declination is south, or in the scale D if it is north, will be found the time of its rising above the horison; and the degree of declination on the scales E and F, according as it is south or north, will point out on the horary circle the time of the star setting. If the rising of the star is known from observation, bring its declination to the time of its rising on the circle of hours, and the index B will point out the time at which it passed the meridian; and its declination on the opposite scale will indicate the time when it descends below the horizon. In the same way, from the time of the star setting, we may determine the time when it rises and comes to the meridian.

The two exterior circles are added to the astrometer, for the purpose of finding the position of the stars and planets

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* The most accurate table of semidiurnal arcs that I have seen, is published in the Tables de Berlin, Tom. III. p. 233.

Description of an astrometer for determining the apparent situations of the stars, &c.

in the heavens. The outermost of these is divided into 360 equal parts, and the other, which is a scale of amplitudes, is so formed, that the amplitude of any of the heavenly bodies may be exactly opposite the corresponding degree of declination in the adjacent circle. The degrees of south declination, for instance, in the latitude of 51°, corresponds with an amplitude of $15\frac{1}{3}$°, consequently the fifteen degrees of amplitude must be nearly opposite to the tenth degree of declination; so that by a table of amplitudes, the other points of the scale may be easily determined. The astrometer is also furnished with a moveable index M N, which carries at its extremities two vertical sights *m n*, in a strait line with the center A. The instrument being thus completed, let it be required to find the planet Saturn, when his declination is 15° north, and the time of his southing 3^h 30′ in the morning. The times of his rising and setting will be found to be 7^h 15′, and 10^h 45′, and his amplitude 24° north. Then shift the moveable index till the side of it which points to the center is exactly above the 24th degree of the exterior circle in the north-east quadrant, and when the line A B is placed in the meridian, the two sight holes will be directed to the point of the horizon where Saturn will be seen at 7^h 15′, the time of his rising. The same being done in the north-west quadrant, the point of the horizon where the planet sets will likewise be determined. In the same way the position of the fixed stars, and the other planets, may be easily discovered.

If it is required to find the name of any particular star that is observed in the heavens, place the astrometer due north and south, and when the star is near the horizon, either at its rising or setting, shift the moveable index till the two sights point to the star. The side of the index will then point out, on the exterior circle, the stars amplitude. With this amplitude enter the third scale from the centre, and find the declination of the star in the second circle. Shift the moveable horary circle till the time at which the observation is made be opposite the star's declination, and the index B will point to the time at which it passes the meridian. The difference between the time of the star's

Description of an astrometer for determining the apparent situation of the stars, &c.

southing, and 12 o'clock noon, converted into degrees of the equator, and added to the right ascension of the sun if the star comes to the meredian after the sun, but substracted from it if the star souths before the sun will give the right ascension of the star. With the right ascensions and declination thus found enter a table of the right ascensions and declination of the principal fixed stars, and you will discover the name of the star which corresponds with these numbers.—The meridian attitudes of the heavenly bodies may always be found by counting the number of degrees between their declination and the index B. The astrometer may be employed in the solution of various other problems; but the application of it to other purposes is left to the ingenuity of the young astronomer.

I am, Sir,

Your obedient humble Servant,

DAVID BREWSTER.

Edinburgh, April 14th, 1807.

II.

Questions and Remarks concerning the best Methods of destroying the Insects which infest Dwellings and Furniture. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Inquiry respecting the means of destroying fleas and other insects.

I LATELY read in a periodical work some enquiries by a "trifling Querist" respecting the best means of destroying or expelling those troublesome insects bugs and fleas, but particularly the latter. I was in great hope of seeing some communication that might have been useful for this purpose, but was much disappointed to read in a later number a flippant sort of answer, that we might prevent their bite by covering our bodies with tar or pitch, and that their bite might be cured by patience and letting it alone. The same means, Sir, will cure the tooth ach.—Some persons suffer very little inconvenience from the bite of poisonous insects.

Inquiry respecting the means of destroying fleas and other insects.

insects, whilst others, from what cause I know not, suffer severely. Unfortunately for myself I am one of these. The bite of a common flea causes a very considerable degree of pain and inflammation; so great indeed as totally to disturb my rest until either the little animal is satisfied, or till I am fortunate enough to destroy it. If knowledge be valuable in proportion to utility, the means of preventing the distress occasioned by the bite of insects is not beneath the attention of philosophers. Cleanliness I know will prevent the incroachment of these vermin; but no one can guard himself from them by cleanliness of his own person, unless he can prevail on all persons with whom he has intercourse to take the same care. I wish some of your correspondents who have pleasure in the study of natural history would bestow some attention on this subject, and communicate to the public the result of their investigation. It is remarkable that different constitutions are so differently affected by the same poisons. The bite of fleas or bugs is insufferably painful to some persons, whilst others are not at all incommoded. The reason of this might be a subject of curious enquiry, But it would be an important comfort to those who suffer severely, to be acquainted with any means of protecting themselves against such distress. The common head-louse is easily destroyed or expelled from the head by combing into the hair a small quantity of white Hellibore. Whether this drug is equally deleterious to the flea, I do not know, but the experiment might merit a trial. Perhaps rinsing the blankets through an infusion or decoction of it, might render them a disagreeable lodgement to any insect. Mercury we know is in every form destructive to the insect tribe, but whether any useful application could be made of it in this case I am unable to determine. I have known the red nitrate of mercury combed into the heads of children for the purpose of destroying vermin, and I believe with complete effect. Many of the solutions of this mineral are so corrosive as might injure the texture of the clothes; but perhaps a very weak solution of the acetite of mercury, suppose a grain to a pint of water, might be used to rince the clothes through, without injuring them, or occasioning to a person sleeping in them any unpleasant effects; yet

Inquiry respecting the means of destroying fleas and other insects.

even this small quantity might be so disagreeable to fleas as to expel them. Sulphur is, I believe, destructive, or at least disagreeable to insects, but I much doubt whether it could be used in any convenient form to answer our purpose. Perhaps the sulphur-water, water containing sulphuretted hydrogen, might have some effect, but it would, in most situations, be too expensive for use. It is said that wormwood is very offensive to fleas. Probably an infusion of it might be very advantageously used to secure us against the intrusions of these troublesome little animals. As it might often be inconvenient to procure the herb fresh, it would be important to know whether the oil, or any preparation of it that could be conveniently preserved, would answer the purpose. Camphor is said to be offensive to bugs; but though I never made any experiments expressly on the subject, yet I think I am enabled by accident to contradict it. It might merit a trial, whether washing the body over with any of these articles would secure us against the bites of fleas, or bugs, or musquitoes, or gnats. With respect to the cure of these bites I can say little. I have heard military men, who have been in warm climates, speak of the custom of laying a cut lemon by the bed-side, and rubbing the part with it immediately on being bit. If any of your correspondents can communicate any useful information on this subject, I shall be one amongst many others who will feel extremely grateful at being relieved from one of the "miseries of human life."

I am, Sir,

Your obedient Servant,

A.

III.

Account of the Method and Advantage of heating Apartments and Manufactories by Steam. By Mr. NEIL SNODGRASS[*].

History and account of the method of heating rooms by steam.

IN April 1798 Mr. Snodgrass was engaged by G. Mackintosh and David Dale, Esqrs. to manage a cotton mill near Dornoch,

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[*] The Society of Arts gave a premium of Forty Guineas for this useful communication.

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Dornoch, in the county of Sutherland. He remained in Glasgow for six months after this, superintending the construction of machinery for the mill. During this period he was led to consider of a cheap method of heating the mill, as he had learnt that fuel was extremely scarce and dear in the country in which the mill was situated. It was evident that none of the methods which he had seen practised could be applied, but at an enormous expence; and his experience had pointed out to him important defects and inconveniences in them all. Having observed a mode of drying muslins by wrapping them round hollow metal cylinders, filled with steam, practised at the bleach-fields near Glasgow, it occurred to him, that by means of a proper apparatus, steam might be applied to heat a cotton mill, or any other large manufactory. It was evident that this not only would be an œconomical mode of producing heat in large works, so far as fuel was concerned, but that it would prevent the danger of fire, to which such works, when heated in the usual manner, are much exposed. He communicated his notions to a number of cotton spinners and others, from whose suggestions he expected assistance. But he met with nothing but discouragement, the project being every where treated lightly, or pronounced to be impracticable. Strongly impressed, however, with the advantages of the plan, the memorialist persevered in his resolution to make trial of it, and ordered tin pipes to be made for the purpose. These he erected in the mill in May 1799. When filled with steam they at once produced the necessary degree of heat; but the pipes, having been damaged in the carriage, proved not sufficiently strong. Indeed the memorialist was immediately sensible, that their position was unfavourable. With a view to some conveniences in point of room, they had been carried up diagonally in one end of the mill, whence the upper sides of the pipes became sooner heated than the lower, which caused an unequal expansion. The water arising from the steam condensed in the pipes in its return to the boiler, and also obstructed the steam in its ascent. In order to remedy these defects the pipes were altered, and erected in a perpendicular position, and certain tubes were connected with them, to carry off the water

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arising from condensation. The whole apparatus, as it stood after this alteration, is represented by the drawing, Fig. 1. Plate I.

This drawing presents a view of an inner gable, which is at one extremity of the preparations and spinning rooms of the mill. On the other side of this gable there is a space of 17 feet enclosed by an outer gable, and containing the water wheel, the staircase and small rooms, for the accommodation of the work. In this space the furnace and boiler are placed on the ground. The boiler cannot be shown here, as it lies behind the gable exhibited; nor is it of any consequence, as there is nothing peculiar in it. It may be of any convenient form. The feeding apparatus, &c. are in every respect the same as in the boiler of a common steam engine. A circular copper boiler, two feet diameter, by two feet deep, containing 30 gallons of water, with a large copper head, as a reservoir for the steam, was found to answer in the present instance. The steam is conveyed from the boiler through the gable, by the copper pipe B, into the tin pipe C, C. From C it passes into the centres of the perpendicular pipes E, E, E, by the small bent copper tubes D, D, D. The pipes E, E, E, are connected under the garret floor by the tubes F, F, for the more easy circulation of the steam. The middle pipe E is carried through the garret floor, and communicates with a lying pipe 36 feet in length (the end of which is seen at G,) for heating the garret. At the farther extremity of the pipe G, there is a valve falling inwards to prevent a vacuum being formed on the cooling of the apparatus; the consequence of which would be the crushing of the pipes by the pressure of the atmosphere. Similar valves, K, K, are placed near the top of the perpendicular pipes E, E; and from the middle one E, the small pipe passes through the roof, and is furnished with a valve at I, opening outwards, to suffer the air to escape while the pipes are filling with steam, or the steam itself to escape when the charge is too high.

The water condensed in the perpendicular pipes E, E, E, trickles down their sides into the three funnels, L, L, L, the necks of which may either pass through, or round, the pipe

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C, into the copper tube M, M, which also receives the water condensed in C, C, by means of the short tubes N, N. The pipe C, C, is itself so much inclined as to cause the water to run along it to the tubes N, N, and the pipe G in the garret has an inclination of 18 inches in its length, to bring the water condensed in it back to the middle pipe E. The tube M, M, carries back the water through the gable to the boiler, which stands five feet lower than this tube. It is material to return the water to the boiler, as, being nearly at a boiling heat, a considerable expence of fuel is thereby saved.

The large pipes are ten inches in diameter, and are made of the second kind of tinned iron plates. The dimensions of the smaller tubes are seen by their comparative size in the drawing, and perhaps they might be varied without inconvenience.

The apparatus erected as here described, has been found sufficiently strong, and has required no material repairs since the first alterations were made. The leading object in the instance under consideration being to save fuel, in order to derive as much heat as possible from a given quantity of fuel, the flue from the furnace, which heats the boiler, is conveyed into common stone pipes placed in the gable. These are erected so as to prevent any danger of fire, in the manner shown in the drawing, Fig. 2. The steam with this auxiliary communicates a heat of about 70° to the mill, the rooms of which are 50 feet long, 32½ feet wide, and 8½ feet high, except the lower story and garret; the former of which is 11, and the latter 7 feet high. The rooms warmed in this manner are much more wholesome and agreeable than those heated by the best constructed stoves, being perfectly free from vapour or contaminated air.

By various experiments it appears, that the expence of fuel is scarcely one half of what is necessary to produce the same degree of heat with the best constructed stoves. The memorialist was the better able to make the comparison, since he had previously had five years experience of cotton mills on what was, at that time, reckoned the most approved plan.

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After having ascertained these results, the memorialist, in 1800 drew a plan similar to that now presented to the Society, and sent it to Glasgow to his employers, who were very doubtful of the success of the scheme. They immediately published the discovery in the Glasgow newspapers, inviting cotton spinners, and others interested, to inspect the plan. In consequence of this public intimation of the method having been successfully practised, a number of cotton spinners turned their attention to it, and adopted it with various modifications, according to the convenience of their mills, or other notions of improvement.

The memorialist afforded to every person who desired it, all the information on the subject which he possessed. His general recommendations were to detach the condensed water, in returning it to the boiler, as much as possible from the steam; and where tin pipes, or others of similar strength, were used, to secure them carefully with safety valves.

There are obvious defects in the application of the principle, as practised in the instance described above. Of some of these the memorialist was perfectly aware at the time of the first construction of the apparatus, though it was out of his power to remedy them; and he has thought it proper to give a detail of the first successful experiment exactly as it took place.

From the pipes being all in one end of the house, the heat was unequally diffused, and a considerable time elapsed, after their being first heated, before it reached the other end of the rooms. But, as the mill had barely room enough for the spinning machinery, it was impossible to erect the pipes in any other situation, or to convey them along the rooms, so as to produce a more equal distribution of heat. This, however, can be so easily effected, when there are no obstacles, such as have been mentioned, that it is scarcely necessary to enter into any detail of the means. It may be barely mentioned that the memorialist has fitted up the apparatus in two cotton mills, which are now under his management, belonging to George Houston, Esq. and Co. of Johnstone, in a manner which completely distributes the heat. In one of these mills, consisting of six stories,

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stories, a lying pipe of cast iron, five inches in diameter, is carried along the middle of the lower story, about two feet from the ceiling, with a small declivity to carry off the water. This pipe heats the story in which it is placed. Tin pipes, 7½ inches diameter, communicating with this lying pipe, are carried up perpendicularly through all the floors to the top of the house at the distance of seven feet from each other, and form a line of heated columns in the middle of each room. The same general plan has been followed in the other mill. But there are several irregularities in the building, which require a little variation of the contrivances for diffusing the heat to every quarter. Some of the rooms having been added since the first erection of the mill, are connected with the main body of the building awkwardly. Into these the steam is carried by lying pipes, slightly inclined, and communicating with the principal apparatus. The steam may afterwards be distributed by other pipes in any way that is thought convenient. The memorialist has found no difficulty in conveying, by such means, the steam necessary to produce the degree of heat required in every variety of situation.

In the former of the last mentioned mills, the perpendicular pipes are connected under the ceiling of the garret by a pipe 2½ inches diameter, slightly inclined, the extremities of which pass through the walls of the house, and are provided with valves opening outwards. A connecting pipe, with similar valves, is placed under the ceiling of the third story. These are intended for the more easy circulation of the steam; but the memorialist found, from experience, that with all these aids, the filling of the perpendicular pipes with steam was attended with some difficulty. The steam, when first thrown in, passes up the perpendicular pipe, nearest to the boiler, and, being specifically lighter than air, occupies the upper part of the apparatus, compressing the air in the lower part of the rest of the pipes. The resistance of the air will thus for a long time prevent the pipes from being completely heated: but this difficulty is easily obviated by having a valve or valves opening outwards, at the lowest part of the apparatus, through which the air, when compressed by the steam, is suffered to escape.

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escape. In the mill just mentioned, the lying cast iron pipe in the first story is carried through the gables of the mill, and furnished with valves for the egress of the air. It is unnecessary to repeat, that the same valves serve for the discharge of the air in heating the apparatus, and of the steam itself, when its expansive force becomes too great. In both mills, each of the perpendicular pipes is provided with a valve, to prevent a vacuum: and in the second mill the lying pipes for carrying the steam into the detached rooms have each two valves, one opening inwards, and the other outwards.

Certificates of five other mills being heated in a similar manner, by the direction of the memorialist, are presented to the Society.

The application of the principle to buildings already constructed, it is presumed, will be sufficiently obvious from the foregoing details. In new manufactories, where the mode of heating may be made a part of the original plan, a more convenient apparatus may be introduced. This will be best explained by a description of the drawing, Fig. 2. which gives a section of a cotton mill constructed in a manner which the memorialist would adopt, were he to apply the steam apparatus to a new building, or any other that would permit such an apparatus from its regular constructions. In an old mill in this place, an apparatus is now erecting by the advice of the memorialist, conformable to this plan, which is likely to be generally adopted in new cotton mills.

The furnace for the boiler is shown at *a*. The flue of the furnace conveys the smoke into the cast iron stove pipes, 1, 2, 3, 4. These pipes are placed in a space in the gable, intirely enclosed with brick, except at the small apertures, 5, 6, 7, 8. A current of air is admitted below at 9, and thrown into the rooms by those openings, after being heated by contact with the pipes. This part of the plan is adopted with a view to prevent, as much as possible, any of the heat, produced by the fuel used, from being thrown away: It may be omitted where any danger of fire is apprehended from it, and the smoke may be carried off in any way that is considered absolutely secure. So far, however, as the memorialist

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memorialist is able to judge, there seems to be little or no danger of fire from a stove of this construction. The greatest inconvenience of a common stove is, that the cockle or metal furnace is liable to crack from the intensity of the heat. By the continuity of the metal from the fire-place, an intense heat is also conducted along the pipes, which exposes them to the same accident. Here the smoke being previously conveyed through a brick flue, can never communicate to the pipes a degree of heat sufficient to crack them. In like manner the pipes, having no communication with the rooms but by the small apertures, cannot come in contact with any combustible substance; and from being surrounded with air, which is constantly changing, can impart only a very moderate degree of heat to the walls. The iron supporters of the pipes may be imbedded in some substance which is a bad conductor of heat, as furnace ashes and lime, &c. The emission of heated air into the rooms may be regulated by valves. As the pipes are not exposed to cracking, there is no risk of their throwing smoke or vapour into the rooms.

The boiler *b*, *b*, is six feet long, three and a half broad, and three feet deep. As there is nothing peculiar in the feeding apparatus, it is omitted. The boiler may be placed in any convenient situation. Where a steam engine is used for other purposes, the steam may be taken from its boiler. The pipe *c*, *c*, conveys the steam from the boiler to the first perpendicular pipe *d*, *d*, *d*. There is an expanding joint at *e*, stuffed, to make it steam tight. The steam ascending in the first pipe *d*, *d*, *d*, enters the horizontal pipe *f*, *f*, *f*, *f*, (which is slightly inclined) expelling the air, which partly escapes by the valve *g*, and is partly forced into the other pipes. The valve *g* being considerably loaded, forces the accumulating steam down into the rest of the pipes *d*, *d*, *d*. The air in these pipes recedes before the steam, and is forced through the tubes *h*, *h*, *h*, into the pipe *m*, *m*, *m*, whence it escapes at the valve *i*, and the syphon *k*. The water, condensed in the whole of the pipes, passes also through the tubes *h*, *h*, *h*, *h*, into the pipe *m*, *m*, *m*, which has such a declivity as to discharge the water at the syphon *k*, into the hot well *n*, whence it is pumped back into the boiler.

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The whole of the pipes are of cast iron, except *m*, *m*, *m*, which is of copper. The perpendicular pipes serve as pillars for supporting the beams of the house, by means of the projecting pieces *o*, *o*, *o*, which may be raised or lowered at pleasure by the wedges *p*, *p*, *p*. The pipes are sunk in the beams about an inch, and are made fast to them by the iron straps *q*, *q*. Those in the lower story rest on the stones *s*, *s*, *s*, *s*, and are made tight at the junction with stuffing. The pipe in each story supports the one in the story above by a stuffed joint as shown at *r*. The pipes in the lower story are seven inches in diameter; those in the higher six inches; those in the other two are of intermediate diameters. The thickness of the metal is ⅜ of an inch. The lower pipes are made larger than the upper, in order to expose a greater heated surface in the lower rooms, because the steam being thrown from above into all the pipes, except the first, would otherwise become incapable of imparting an equal heat as it descends.

There is no necessity for valves opening inwards in this apparatus, the pipes being strong enough to resist the pressure of the atmosphere.

The cotton mill is 60 feet long, 33 wide, and four stories high, the upper being a garret story. In the engraving five parts out of nine in the length of the building are only shown. The apparatus will heat the rooms to 85° in the coldest season. It is evident that, by increasing the size, or the number of the pipes, and the supply of steam, any degree of heat up to 212° may be easily produced. It may even be carried beyond that point by an apparatus strong enough to compress the steam; this, however, can seldom be wanted. At first it was objected to this construction, that the expansion of the pipes, when heated, might damage the building; but experience has proved, that the expansion occasioned by the heat of steam is quite insensible.*

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* Certificates from Mr. George Mackintosh, and Messrs. Henry Monteith, Bogle and Co. of Glasgow; Mr. George Houston, Messrs. Robert Hodgart and Co. Messrs. John Fife and Co. and Mr. John M'Naught, of Johnstone; Mr. James Boyle, manager for Messrs. M'Farlam, Black and Co. at Gryse-Mill; also from Mr.

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The memorialist thinks it would be improper, in addressing so intelligent a body as the Society ef Arts, &c. to expatiate on the various economical purposes to which the principle, which he has been able but imperfectly to unfold, may be applied. In abler hands it may be found susceptible of improvement, which he cannot anticipate.

NEIL SNODGRASS.

IV.

Experimental Inquiry into the Nature of Gouty and Gravelley Concretions. By THOMAS EGAN, *M. D. FRS**.

THE urine of gravelly patients, when fresh rendered, nay, after standing many hours, in a temperature of sixty degrees, is relatively more acid than the healthy; sometimes as much so as the gouty; and frequently continues so, even after depositing its gravelly matter. An exception to this, however, sometimes occurs in gouty habits; their urine depositing copiously this acid substance, and yet manifesting no increased, but sometimes rather decreased acescency; for, with them, a considerable diminution of the quantity of the usually excreted super-acidulated phosphoric salt often takes place, as shall be fully explained upon another occasion.

Having premised these observations, it is now time to consider what effects acid substances are productive of, when mixed, out of the body, with this very complicated liquor. And here, to prevent repetition, I will observe, that that generally used, was rendered fresh in the morning, in the quantity of from three to four ounces, (unless otherwise specified;) being that most easily retained at one time in the bladder. The quantity of acid extremely small,

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Mr. William Kerr, for the Lochwinnoch Spinning Company, confirm the utility and success of Mr. Snodgrass's method, and attribute to him the credit of first applying steam to the purpose of heating manufactories.

* Extracted from a longer Memoir in the Irish Transactions, 1805.

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small, for obvious reasons, and seldom increasing its acescent properties (as ascertained by the usual tests) beyond what frequently occurs, in the urine of those who use acescent drinks, or are afflicted with gout or gravel. A standard quantity was always laid by for comparison; and the temperature from sixty to seventy-five degrees, being in autumn, 1799. And to begin with the vegetable acids.

Exp. 1. To four ounces of the urine of an adult, was added one drachm of common acetous acid, which (like every other acid) caused no immediate change in it; but, in a very short time, and before it cooled down to the temperature of the atmosphere, some extremely minute shining spiculæ, observable only by a lens, were seen floating in it: these gradually increased in number and size, began to reflect the light, and, from being perfectly transparent, soon became coloured, to settle upon the usual cloud, or *nubecula*, which now began to form, adhere to the sides of the glass, and partly fall to the bottom, in the shape of small bright red crystals. In the standard, after twelve hours, nothing more observable, than the usual *nubecula;* nor was there any sign of crystalization, or separation of uric acid, even after twenty-four.

Exp. 2. To the same quantity of adult urine, were added one drachm and half of acetous acid, which caused a more copious separation and crystallization of this substance, with the foregoing appearances. None observable in the standard after twenty-four hours.

Exp. 3. To four ounces of urine of a healthy child, who never was observed to pass gravel, and of the usual degree of acidity, was added one drachm of acetous acid, which soon caused an evident and copious separation of crystallized uric acid. The crystals were, however, not quite so coloured; the urine of children not being so much impregnated with the urée, or colouring matter. No such appearance in the standard after twelve hours or more.

Exp. 4. To four ounces of adult urine, rendered very soon after a tea breakfast, and nearly in a state of *urina potus*, was added one drachm of acetous acid. After three hours, a crystallization of minute sandy particles took place. None in the standard, even after three days.

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Exp. 5. Thirty drops only, of acetous acid, were added to four ounces of the urine of a gouty patient, æt. sixty, and who sometimes felt some slight gravelly tendency. A very copious precipitation of this matter quickly took place. Some observable in the standard, also, the next day.

Exp. 6. To three ounces of healthy adult urine, were added a few drops only of citric acid. A distinct crystallization, but extremely minute, took place. No appearance of any in the standard, after many hours. The experiment was repeated with one drachm of filtered citric acid, which only hastened the separation, and increased the quantity of crystalline matter.

Finding, by these experiments, and numberless others, with a detail of which it would be unnecessary to take up the time of the Academy, that the acetous and citric acids, blended with the urine, separated its uric acid in a crystallized state; I thought it might be interesting, to investigate what the effect of the tartarous acid might be: being that, which, in an uncombined, and partly combined state of acidule, as in the acidulous tartarite of potash, chiefly prevails in the wines and beverage of those countries most subject to these complaints.

Exp. 7. To four ounces of healthy adult urine, were added some drops only of pure tartarous acid. To the same quantity, one drachm of acetous acid; which brought them nearly to the same standard of acidity: a circumstance always attended to in the comparative trials with different acids. In that with the tartarous acid, the crystals were not only larger and darker coloured, but exceeded in quantity any thing before observed. In that with the acetous acid, a much smaller proportion of minute crystals took place.

Exp. 8. To four ounces of urine, were added two drachms of a filtered solution of acidulous tartarite of potash, of the temperature of 55 degrees. The usual separation and crystallization took place, in large proportion: the crystals, however, much smaller, and less coloured, than those with the uncombined tartarous acid. The two last experiments, frequently repeated, presented the same results.

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Exp. 9. The result of the above experiments having led to some doubt, as to the good effects of the carbonic acid gas, so much, at one time, recommended by Doctors Percival and Saunders, previous to its more modern alkaline combination, in our mephitic, as well as super-aerated soda waters.

Into the middle part of Nooth's apparatus, were introduced four pounds of fresh rendered healthy urine, and exposed to a stream of carbonic acid gas. After a few hours, a copious and beautiful precipitation of uric crystals took place, (notwithstanding the constant agitation, from the transmission of the gazeous bubbles,) larger than any I before observed, that from the tartarous acid excepted. In a standard quantity, no distinct crystallization, even after two days. A repetition of the same experiment afforded similar results.

Exp. 10. Finding the carbonic acid gas productive of similar effects, with the other acids hitherto examined; it was natural to inquire, how far its combination with the portion of alkaline matter, contained in our mephitic and soda waters, so highly surcharged with it, may prevent a separation of this uric acid.

Half an ounce only, of the common soda water of the shops, prepared by Mr. Kinsley, was added to four ounces of healthy urine. A similar quantity was impregnated with carbonic acid gas. In the former, after forty-eight hours, or more, no more than the usual *nubecula:* nor could a single crystal be discovered, even by a magnifier. In the latter, an early, copious, and beautiful crystallization. On the result of this experiment, frequently repeated, with various proportions of the mephitic alkaline water, I shall afterwards have occasion to make some remarks.

Though the mineral acids, in an uncombined state, enter not into the matter of our diet, and are no longer considered as lithontriptics, since the notion of the earthy nature of these concretions has been abandoned; yet, as they are sometimes prescribed with other indications, I thought fit to extend my researches (though in a summary way) to them also.

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Exp. 11. To sixteen ounces of urine, were added eight drops of very dilute sulphuric acid. To a similar quantity, two scruples of citric acid, to bring them to nearly the same standard of acidity. After a very short interval, in that with nitric acid, the usual appearance of transparent floating moleculæ reflecting light, and gradually becoming larger, were observed, and began to adhere to the glass: whilst in the other, after five hours, no such appearances took place. Yet, after forty-eight, here also a precipitation took place, of smaller crystals, and less in quantity; for, being collected on a filter, and carefully dried, they weighed only two grains; whilst the former amounted to three. And this is nearly the largest proportion I ever found the above quantity of healthy urine to contain.

Exp. 12. As the nitrous acid is one of the most active solvents of this matter, out of the body, I was curious to ascertain, whether. in the very dilute state in which it must reach the kidneys and bladder, (where its action must have been facilitated, by the actual state of solution of this substance,) it would manifest its powers, in preventing its separation.

To three ounces of urine, rendered a few hours after breakfast, and, of course, scarcely acid, were added five drops of weak nitrous acid; which did not seem to add very materially to its ascescent properties.

To a similar quantity were added four scruples of acetous acid. In less than an hour, the former deposited a distinct quantity of gravelly matter, in considerable proportion. This, perhaps, we should not be surprised at, when we consider how the action of this acid, in that fluid, may be determined by superior affinity. In the latter, the separation did not take place for a considerable time after. We see, then, that the nitrous acid speedily and powerfully precipitates this acid substance.

Exp. 13. To six ounces of urine, shewing a strong acescent quality, were added only three drops of strong marine acid. A cloudiness and transparent granular precipitation took place, followed by the formation of extremely minute gravelly concretions, which, even after two days standing, did not assume so red a tinge as that with

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vegetable acids. This may, probably, depend upon some action of this acid upon the urée, or colouring matter: but, as to the smallness of the crystals, that evidently depends upon the more speedy precipitation, throwing them down before they can assume their natural size, and leaving but a shade of difference between the crystalline and pulverulent deposits.

Exp. 14. From the above, then, we are satisfied, that the vegetable and mineral acids cause a premature separation and crystallization of the lithic contents of recent healthy urine: but it may be observed, that this only takes place, under circumstances not at all applicable to the living system; viz. a much inferior temperature; and, in some instances, a contact with the atmospheric air: two powerful promoting causes of crystallization in general, but more especially of the less soluble salts. To determine, therefore, this most essential point:

To six ounces of cold but recent urine, (in a well closed phial,) were added five drops of very dilute nitrous acid, which were placed on a sand bath: temperature varying from 80 to about 100 degrees at most. The same quantity, with similar precautions, but without addition, was laid aside, in the laboratory, as a standard: temperature 56 degrees. After a very short interval indeed, and almost as soon as the urine acquired the temperature of between 80 and 90 degrees, small shining granular particles were observable with a magnifier, began gradually to settle upon a broken kind of *nubecula*, or rather *nubeculæ*, and to acquire colour and size, though carried up and down the liquor, which was in constant agitation. This experiment again twice latterly repeated, and always with the same result, (care being taken to keep the temperature, as nearly as possible, for a few hours, between 90 and 100 degrees,) afforded one of the most pleasing objects imaginable; viz. the formation of this crystalline matter, under all the disadvantages of elevated temperature, and constant agitation, from (I may almost say) their primordial moleculæ, to the accomplishment of their full size. And here, indeed, they are most beautiful, and not to be distinguished from these spontaneously deposited.

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The whole experiment strikes us strongly with a semblance of what probably passes, under similar circumstances, in nature; and reminds us of the danger attendant upon acid impregnations, more particularly at bedtime, when the urine, by many hours retention and quiet, has ample time to deposit its uric acid contents in the bladder. From it, also, we learn, that the temperature of the human body, in place of retarding or preventing (as might be expected *a priori)* these pernicious effects, rather promotes them, and that to a considerable degree.

But whilst we endeavoured to establish this point, from practical observation as well as experiment, we seem to have entirely forgot, that the urine itself is an acid liquor; and that, therefore, if acids were so prejudicial, it is not probable, that the provident wisdom of nature would commit the discharge of this necessary excretion to a fluid, which, by prematurely separating it within the body, would completely defeat the object of her humane attention. And would she not, in the infinity of her resources, dispose of it by some less objectionable emunctory?

I would, in the first place, observe, that though healthy urine manifests the properties of an acid liquor, it is in the very smallest possible degree; so much so, that though mentioned long since, by Moraung, Coldevillars, and other surgeons, yet it was not, either chemically, or medically, acknowledged to be so, until the time of Scheele, who finally established this point, as well as the nature of the prevailing acid. And, secondly, that nothing can be more erroneous, than the opinion, which so long prevailed, that the phosphoric acid existed in it, in a naked or uncombined state. It is now well established, that it is only in that of a weak acidule, or acidulous phosphate of lime, very little short indeed of the point of saturation; and hence the weakness of its action, as an acid liquor: for were it not for litmus, and some of the more delicate of the vegetable blues, we would have been, even to this day, ignorant of this property; so very feeble indeed, that it will often not affect an infusion of red cabbage, whilst it turns with litmus, and, sometimes, but feebly, with this most delicate of all acid tests. A single drop of phos-

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phoric acid was added to one ounce of distilled water. Of this weak acid impregnation, one drop was sufficient to turn the infusion of litmus of as clear a red as the mineral acids do; whilst seven of urine manifested but very weak effects of acidity, and required some time to shew any. If the urine, therefore, does not exceed its natural standard of acidity, we have nothing to apprehend. And here, indeed, we must again admire the wonderful wisdom of Providence. The occasion (may I be allowed to say so, and that, too, before so competent an assembly?) required some chemical discrimination. It was necessary to carefully provide for the expulsion of the recrementitious part of the osseous fabric (which is very considerable) out of the system: but as this salt is insoluble in an aqueous vehicle, such as the urine, nothing more would be necessary, to obviate this difficulty, than a certain degree of super-saturation, or state of acidule, which would more effectually provide for its solubility, and its elimination. But by going thus far, whilst it attended to one excretion only, it would have entirely forsaken its charge of another, committed also to this fluid; and, by this degree of super-saturation, precipitate, retain in the system the uric acid, and occasion as frequent an occurrence of gravelly and calculus complaints, amongst mankind in general, as now occurs among the gouty. It, therefore, prudently formed that degree only of acidulous phosphat of lime, which, though insoluble out of the body, was sufficiently soluble, when assisted by its temperature. Nay, even for wise purposes, it has given a degree of latitude to this temperature, which, though narrow and confined indeed, is sufficient for its purposes: but where it precisely terminates I am not at present prepared to say, though so easily determined.

Let us now, for a moment, consider how far any morbid deviation, from this healthy standard, (which sometimes happens,) may throw light on this subject. The most considerable, that I am acquainted with, occurs in the instance of gouty urine, rendered towards the decline of the paroxism. A single drop of this, though in a turbid state, affects the vegetable blues, with an energy, equal, or, perhaps, superior, to that of the strongest acetous acid; and

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and requires a very considerable increased proportion of lime-water to decompose it, for obvious reasons. This we find always depositing, sometimes from the bladder itself, but, generally, before it has entirely parted with its natural temperature, a very large proportion of a reddish brick-dust like sediment, (a welcome harbinger to gouty patients,) gradually declining, and keeping pace with the alleviation of symptoms, and the progressive return of the urine to its natural degree of acidity. This sediment, Scheele, Bergman, and Fourcroy, consider of the uric acid kind: and so it (but in part only) undoubtedly is; being in a smaller proportion than they were aware of. For, considering that the enormous quantity, rendered in a few days, was incompatible with the known minute proportion of this acid matter in urine, I was determined to make the following experiment. To a considerable quantity of it, desiccated and well edulcorated with distilled water, were added three ounces of a weak alkaline lixivium; which, after a few hours digestion, completely discoloured it, acquired a golden yellow colour, a sweetish taste, and, on the addition of a few drops of dilute marine acid, precipitated a copious sediment of whitish minute needle-shaped crystals, of a silky appearance.

To this precipitate, well edulcorated, was added, by degrees, about one ounce of weak nitrous acid, which acted on it, with effervescence, and nearly took up the whole. This solution, being set to evaporate, began to redden the fingers, and other animal matters; no doubt, therefore, could subsist, as to its nature. To the remainder, which seemed very little diminished, and only deprived of colour, were added two ounces of dilute marine acid; which, after some time in digestion, nearly dissolved the whole: and, finding this acid solution precipitate with lime-water, oxalt of ammonia, and fixed alkali, it must have been phosphat of lime. This forms, then, by far the largest proportion of the gouty sediment, which is coloured by the precipitated uric acid. Such, also, is the result of Crookshank's experiments; and so we should expect to find it, as I shall endeavour to point out, on a future occasion.

Let us now consider, how far these analytical results

may

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may be confirmed, in the synthetic way; having resolved, that experiment, as far as applicable, should form the basis of any opinions, offered in this essay. The phosphoric being the native acid, prevalent in urine, it was interesting to determine, whether, by the artificial super-addition of it, so as to bring this fluid to the standard of the gouty, we might not produce effects, somewhat analogous to what occur there.

Eighteen ounces of urine were divided into three equal parts. To the first were added, five drops of phosphoric acid; to the second, ten; and to the third, fifteen. In the first, the magnifier very soon discovered minute floating moleculæ, gradually assuming the crystalline form, &c. as so often before described. In the second, the same appearances, but more immediately and copiously produced. But in the third, so considerable, as to excite my astonishment. For here, besides the same extremely minute crystals, which adhered to the entire sides of the phial, the bottom appeared covered with a mixture of crystalline, and red pulverulent matter: the latter in a great proportion, and, probably, prevented from crystallization, by its hasty deposition. Here, then, that encreased proportion of calcareous phosphat and animal gelatinous matters, (which always takes place in gout, and could not be expected here,) would seem only wanting, to form a sort of synthetic approximation to the gouty sediment.

The unusual proportion of deposited uric acid, in this experiment, created some suspicion, that the phosphoric acid might, by a combination with some of the principles of this very compound fluid, give rise to some artificial formation of it on this occasion.

To the filtered liquor, therefore, of Number 3, were again superadded five drops, which, in twenty-four hours, caused a farther separation of a very few crystals only.— It was filtered a third time, and eight drops more added; but without the smallest appearance of a single crystal, after four days. The additional acid, then, only more effectually and speedily determined the separation of the quantity, naturally contained in urine: its more divided pulverulent appearance adding considerably to its volume.

It

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It now only remained, to demonstrate the identity of these various precipitates, with the naturally deposited matter of gravel. For, though it could not be well mistaken, for any other saline composition in urine; yet, as external characters are, even in the hands of a Romé de Lisle, or an Abbe Haüy, fallacious, the following, and concluding one, on the subject of acids, was instituted.

Exp. 15. To two drachms of this artificial gravelly matter, was gradually added one ounce of nitrous acid; which acted on it, with effervescence, and dissolved the whole, with the exception of some small, floating, floculent, animal particles, so well described by Bergman.

The evaporated solution reddened the skin, and, after some time, deposited crystals of oxalic acid; as happens in all concentrated nitrous solutions of calculi, of the uric acid kind. To another small quantity, was added some pure alkaline lixivium; which very soon took it up, became coloured, sweetish, and deposited the usual silky crystalline sediment, upon the addition of acetous acid. No doubt, therefore, could remain, as to its identity, with that naturally deposited

And here, though irrelevant to my present object, and merely with a view to excite the attention of the faculty, may I be permitted to ask, how it happens, that, in the very worst kinds of typhus fever, there is very little diminution of the secretion, or excretion of the acidulous phosphat of lime? as appears by the acidity of the urine, lime-water, and the *quantum* of precipitate, afforded by the oxalic acid: whilst a very considerable one of the uric acid takes place, and continues so, until nearly the termination of the disease, when it begins gradually again to manifest itself; first, by the usual tests only; but presently, upon the crisis taking place, in such quantity, as to become insoluble; and, therefore, quickly precipitates, (with some additional mixture of calcareous phosphat, and animal mucilaginons matter,) under the form of our critical sediment or deposit? Or, are we not here, again, to admire the wise economy of the Author of nature, which, by keeping up the considerable and necessary bony excretion of the system, prevents the dangerous accumulation of it, which must ensue,

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sue, *from* its retention, during the long protracted period of many fevers? I might here offer some conjectures, in explanation, but will reserve them for another place.

Having already trespassed so much upon the indulgence of the academy, I shall here content myself with briefly stating, that, from the above experiments and observations, we may presume to say, acids of every kind are prejudicial, and give rise to the formation of gravelly and calculus affections, by causing a separation, and crystallization of the lithic acid contents of urine, within the body: not pretending, however, to deny the existence of other causes, inherent in the system itself, occasionally productive of similar effects, as has been already observed.

V.

An Essay, Physiological and Experimental, on the Effects of Opium on the living System. By WILLIAM ALEXANDEK, *M. D*[*].

Effects of opium on the living system.

IN this paper, which I have the hononr to submit to the consideration of the Society, I propose to enter into an examination of two questions, *viz.* 1st. *Can the effects exerted upon the living system, by the operation of Opium, be accounted for, without the agency of the nervous system?* 2d: *What is the nature of that operation, whether sedative or stimulant?*. This subject I do not select either because I have some new doctrines to establish, or because the generally received opinions concerning the operation of opium, require additional support, but rather because in the discharge of a duty for a long season neglected, I am obliged to have recourse to those means, which the present opportunity allows me.

In this disquisition, it may be considered, that I enter upon the investigation of a subject, which has already been rendered barren by the diligence of preceding enquirers, and that consequently nothing of novelty can be expected. It

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[*] Abstracted and abridged from the Manchester memoirs, New series. Vol. I.

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It is not under this expectation that I take up the pen, for how much soever may have been effected, something yet remains to be done by the diligent and patient enquirer, and, though nothing could be gained beyond a confirmation of established opinions, yet if this be done through the means of accurate and repeated experiments, something is added to the stock of information, and it must be considered at the least as possessing a relative value.

These experiments are not however destitute of some novelty in the arrangement, and they will be found to exhibit, in a clear analytical succession, the effects produced by opium upon the different parts of the animal machine. But it is not clear by any means, that the physiologists of this day are agreed upon many points, which will be brought forwards in this essay, and more is required to be done, before the subject can be considered as exhausted.

The humoral pathology, which had for a long space of time occupied the schools of medicine, had no sooner been called in question than a variety of opponents arose in every quarter against it; the new opinions being clothed in professional authority and enforced by the learning and genius of several private teachers, the tide of opinion flowed in a contrary direction, and it became the fashion to account for all, or most of the deviations from a state of health in the animal body, from some primary alteration in the condition of the *solids*. In many points the advocates for the new doctrines were notwithstanding at issue with each other, and the memorable contest betwixt Haller and Whytt, respecting the origin and nature of irritability, opened the physiologist new sources of enquiry and laid new foundations for future improvement. The agency of the nervous system, which was still necessary for the explanation of most of the phœnomena upon the theory of diseased solids, began at length to be exploded by the advocates of another yet more refined and simplified, which the creative genius of John Brown ushered into the schools of physic.

This doctrine rejecting the explanation of diseases upon partial and confined theories, attempted to refer all the various changes in the human body to one general law.

He

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He maintained the existence of a principle in the animated body, which he denominated excitability; That this principle was characteristic of life; that action was the consequence of the operation of certain powers upon this principle, health the consequence of the due and proper operation of these powers, and disease the effect of the abundant or deficient action of these powers.

In this state of things the very accurate and most philosophical thesis of Dr. Goodwin, upon the cause of death from suspension and submersion, made its appearance, in which he plainly proved the existence of a primary change in the condition of the blood; that this condition was sufficient, and indeed necessary to occasion death. About this period also the experiments of the celebrated Italian philosopher, Fontana, attracted considerable attention and became the subject of much discussion. He contended from numerous experiments, that opium was a power, which exerted a direct influence upon the blood, or that the blood was a necessary agent to communicate its operation to the living and irritable fibre, and without the circulation of which, the usual effects of opium could not take place. His experiments, which excluded the agency of the nerves altogether in producing the general effects, resulting from the exhibition of opium, afforded considerable support to those who maintained some new doctrines of irritability*.

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* These physiologists rejecting the nosology and practice of Dr. Brown as incompatible with his fundamental principles, but adopting these, and using the borrowed term of irritability instead of excitability, attempted to establish a new hypothesis, by explaining all the changes, which the body underwent in a state of health and disease, upon an alteration in this principle. The experiments of Fontana, which went to deny the influence of the nerves, coinciding with this new hypothesis were eagerly embraced by them.

The manner in which these physiologists explained the consumption of irritability upon the application of a stimulus, without the agency of the nerves was somewhat curious. They supposed the principle of irritability was like the matter of heat, diffusible over every part of a body endowed with it, that when any portion of it was destroyed by the action of a power applied to any part,

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This property was considered not only as not being derived from the nervous system, but capable of being encreased, diminished, or exhausted by the application of external powers, which had no effect upon the nervous system, and that it was, to use the words of Dr. John Brown, as applied to his principle of excitability, "Una toto corpore et indivisa proprietas."

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To ascertain how far some of these opinions were consistent with the laws of the animal œconomy, I instituted a set of experiments, which formed the subject of an inaugural dissertation, published in the year 1790. It appeared to me in consequence of that investigrtion, that several of the above-mentioned opinions, *viz.* That opium did not act upon the nervous system; that it acted upon the blood; that its effects conld be extended by means of the one and indivisible property of irritability, had been founded upon reasons which were very unsatisfactory.

This publication being calculated principally for the meridian of Edinburgh, was confined to that place, and the question, taking in a general point of view, was left undetermined.

Since that time I find, from the perusal of a work, called "Medical Extracts" written by a gentleman of some ability, but of more *imagination* than judgment; that the opinions of Fontana are not only sanctioned by respectable authority, but are considered as generally known, understood and acted upon. I have, therefore, thought it necessary to collect into a short point of view the facts, related by Fontana, and the general conclusions he drew from them, and to compare them with the principal facts, established by the investigation above alluded to.

"I* destined, says Fontana, 300 frogs for these experiments, and by means of pincers and scissars, I laid bare

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the expenditure, thus occasioued, was supplied by the influx of a new quantity from the general stock in the system; thus the continued action of a stimulant power, keeping up a continued expenditure, there would be a succession of new influxes until the whole irritability of the body was consumed by the repeated wants of that part to which the destructive agent was applied.

[*] Medical Extracts 630. Vol. 3.

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the crural nerves in such a manner, that they were entirely free of every other part, and obtained about eight or ten lines of nerve totally clear and in some very large frogs even more. I then let fall the nerves of each thigh into a small hollow glass, which received them in such a way that I can fill each glass with a fluid of any kind without its touching the adjacent muscles. I usually have been able to put into these glasses, such a proportion of whatever I wish to try on the nerves, as to cover the greater part of them with it, without its being possible for any of the liquor to find its way to the thighs and mix with the blood. In this way I can make a comparison betwixt the nerves, that are envenomed and those that are not; compute the time they continue to contract the muscles, and judge of the vivacity of the motions."

"At the end of the first ten minutes I stimulated the medicated nerves, *i. e.* those, to which the solution of opium was applied, and those which were not medicated, and found that the two extremities, the right as well as left, contracted with the same force and vivacity."

"At the end of twenty minutes, I tried the stimulation, and could perceive no sensible difference betwixt the motions of the two feet, which were almost as lively as those in the first experiment."

"At the end of thirty minutes, the motions of the two feet were feebler, but alike in both."

"At the end of forty minutes, the feet scarcely contracted, but their distinct muscles were clearly seen to contract, when the crural nerves were stimulated, and the motions of these muscles were equally lively in each foot."

"At the end of fifty minutes, the motions were very small 'from compression of the nerves,' but alike in both sides."

"At the end of eighty minutes, there was no longer any motion to be observed in several of the frogs, in whatever way I stimulated either the crural nerves that were medicated, or those which were not so."

"I can conceive," adds Fontana, "nothing more decisive and more certain than from this series of experiments, that the action of opium is not directly on the nerves."

2dly.

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2dly. Again,* Fontana immersed the hearts of various animals immediately taken from the thorax, into a strong aqueous solution of opium, infusion of bark and simple water, of equal temperatures, and found that these organs were deprived of irritability, and that they ceased to contract, or to be capable of being excited to contract, equally soon on immersion into water as into a solution of opium or infusion of bark.

3dly. He next injected an aqueous solution of opium into the jugular vein of several rabbits, and found that it produced death instantaneously; from this he concludes, as the heart is not furnished with nerves, and having proved that the solution of opium does not exhaust the irritability of the heart, that it must occasion death only by producing an alteration in the condition of the fluids.

The experiments with the 300 frogs, as related by Fontana, I repeated, though upon a smaller scale, yet sufficient to ascertain the truth of it. I followed the method described by Fontana, and I found the fact to be correctly as he relates it; the divided extremity of the crural nerves, bared for the space of half an inch, and immersed in solutions of opium of various degrees of strength, was not more affected than if the same nerves had been immersed in water, and the irritability of the muscles, to which they were distributed, was not in the least degree more altered.

Although I admit the accuracy of these experiments, I am inclined to call in question the sufficiency of them for the purpose they were designed. There is a considerable difference betwixt the sentient and the divided extremity of a nerve. This operation, even if the structure of the divided part was capable of receiving and communicating impressions, must in a great measure have had the effect of destroying its sensibility, and though the solution was not only applied to the divided extremity, but also enveloped the surface of the nerve for a considerable distance, this surface must also have lost in consequence of being separated from the muscles by "scissars and pincers" so much of the usual quantum of sensibility as to be unequal to transmit any effect produced upon it.

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* Fontana on Poisons, Vol. II. p. 352–364. French edition.

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Respecting the second series of experiments, they are so contrary to all that repeated experience has taught, so contrary to the observations of Haller, Whytt and Munro, who, notwithstanding the difference of opinion they held, on some points connected with the operation of opium, unequivocally agreed on this head, (viz. that it destroyed the irritability) that I cannot but conjecture, some unobserved circumstances must have diverted the usual accuracy of Fontana from its natural bias.

The conclusion drawn from the third series of experiments rests partly upon the accuracy of the second, and partly upon the supposition that the heart has not any nerves, which is concluded because the knife of the anatomist has not discovered them; but except this opinion is maintained upon some other ground, it can be considered only as a *petitio principii;* the want of detection proves nothing either way, as it is nothing more than an argument of non-existence drawn from invisibility. Further, the experiment proves too much; the animal died instantaneously, on the injection of the solution into the jugular vein; the circulation must of course be interrupted; by what means was this sudden, this momentary effect communicated to the distant parts of the animal?

I have thus stated the proofs and arguments founded upon them, adduced by Fontana, as accurately and at as much length, as the limits of this paper will allow; let us now see how the case stands when reduced to the test of experiment.

Does Opium act upon the Irritability of the Muscular Fibre?

Exp. 1.* The heart of a frog was immersed into half an ounce of an aqueous solution of opium, in the proportion of half a dram and six grains of opium to one ounce of water, of the temperature of 44°, whilst contracting 25 times in a minute. Two minutes after immersion, it contracted only 15 times in a minute: after 8 minutes the contractions had ceased, and could not be excited again by any mechanical stimulus.

Exp. 2.† The heart of a moderate sized rabbit whilst contracting 23 times in a minute, was immersed into an ounce of the above solution, of the temperature of the room; four minutes after immersion, it exhibited eighteen contractions in a minute: ten minutes after immersion, six or eight contractions, and after twelve minutes had entirely ceased, and could not be excited anew to contract.

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* Vid. p. 5, Inaugural. Dissertat. Exp. 1.

† Vid. Inaug. Dissert. p. 7. Exp. 4.

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Exp. 3.* Another heart immersed in an ounce of a stronger solution, only exhibited three or four strong contractions, on the period of immersion, and afterwards was irritated in vain.

Exp. 4.† The heart of another rabbit was placed in a wine glass and three drams of the strong solution poured on, whilst contracting 50 times in a minute: after three minutes spontaneous contractions had ceased, but irritated with a needle a few contractions were excited: after the lapse of five minutes no contractions could be excited.

In order to examine how far the opium contained in the solution contributed to produce the above rapid exhaustion of irritability,

Exp. 5.‡ The heart of a frog, contracting 24 times in a minute, was placed in half an ounce of water, temperature 41°. It continued to contract in the water for 16 minutes, and when removed, contractions could be excited in the organ, by mechanically stimulating it, for the space of *six minutes longer.*

Exp. 6.§ The heart of a rabbit contracting 50 times in a minute, was placed in water of the temperature of the room. It contracted spontaneously for 20 minutes, and when removed continued irritable for the space of 10 minutes longer.

Can the Effect of Opium be communicated to distant Parts independent of the Assistance of the Circulation?

Exp. 7.‖ The sternum of a frog was carefully elevated and the heart removed, forty drops of the strong solution were injected into the stomach. In 15 minutes the animal

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* Vid. p. 10. Inaugural Dissert. Exp. 7.

† Vid. p. 10 & 11. Exp. 8.

‡ Vid. p. 6. Exp. 3. Inaug. Dissert.

§ Vid. p. 8. Exp. 6. ‖ Vid. p. 28. Exp. 20.

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was stupified and paralytic, in 20 minutes convulsed: after 40 minutes, voluntary motion had ceased: after an hour and ten minutes it was dead and the irritability in all the muscles was destroyed.

Exp. 8.[*] Thirty drops were injected into another frog, after the removal of the heart; it lived an hour and 15 minutes, and after death the irritability was exhausted.

Exp. 9.[†] Twenty drops were injected into a third: It lived an hour and twelve minutes, and the state of irritability was the same as in the preceding.

Does the Quantity of Opium sufficient to occasion Death, effect this by inducing a Change in the Condition of the Blood?

Exp. 10.[‡] By some former experiments, No. 17 and 35,[§] it had been found that 33 drops of the solution of opium injected into the jugular vein of a rabbit would occasion death in the course of a few minutes, and exhaust the irritability of the muscular fibre. Another rabbit was selected, and 33 drops injected into the crural vein; no other effect resulted from this but some degree of stupefaction. Twenty-six minutes afterwards 33 more drops were injected into the crural vein of the other limb.

The animal in a short time became more languid, but was not convulsed; its pulse was rendered more slow and feeble, at the period of 36 minutes from the injection into the first crural vein.

Seven hours from the first injection, the animal was convalescent, and the day following it fed as usual.

The occasion did not offer to make a computation of the quantity of opium which would be necessary to kill a rabbit when introduced by a crural vein, but the omission of this does not detract from the force of the evidence which the above experiment supplies, that the cause of the death of the animal, when the solution is introduced by the jugular vein, must arise from some other state, than a change in the condition of the blood, and that the effect of opium,

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[*] Vid. Inaug Dissert. p. 29. Exp. 21.

[†] Vid. p. 30. Exp. 22

[‡] Vid. p. 74. Exp. 49.

[§] Vid. p. 20 & 55.

must

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must have been extended over the entire system, by other means than the circulation; for, what reason can be given why the mass of fluids should not be altered, when the solution was introduced by the crural as well as by the jugular vein; but upon the other theory, the solution of this difficulty is easy, and accords with the whole series of experiments.[*]

The life of a rabbit cannot be sustained a minute without the action of the heart; when the solution of opium is injected into the jugular vein, it is applied to the inward surface of the heart, mixed with a very small quantity of blood, and can then exert effects upon that organ as instantaneously as if the heart was immersed in it, as in experiment 2d [†], the action of opium being thus directed against the irritable fibre, the exhaustion of that part would immediately succeed, of course the animal must die; but when the same fluid is injected into the crural vein, it does not reach the heart until it has been mixed and diluted with a very considerable portion of blood, so that no quantity of blood which the heart could contain during one period of dilatation, would be impregnated with any great quantity of the solution of opium. The consequences therefore, which followed from the injection of opium into the jugular vein, supposing that it acted immediately on the heart, could not in this instance he expected to take place.

Does Opium act upon the Nervous System?

Exp. 11. [‡] A triangular piece of bone was taken from the cranium of a frog, and the dura and pia mater removed; eight drops of the strong solution were injected upon the brain; a few drops were lost. In one minute the animal was convulsed, in three minutes it was dead. On examination the irritability in every part of the voluntary muscles was destroyed, neither compression of the nerves nor mechanical irritation could excite any contractions in them. The heart had not lost its motion.

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[*] Vide Inaug. Dissert. p. 119. Note C.

[†] Vide Inaug. Dissert. p. 17. Exp. 14.

[‡] Vide Inaug. Dissert. p. 49. Exp. 29.

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Exp. 12[*]. In another experiment of the same kind, the animal was immediately and generally convulsed, and was dead in one minute. When the heart was exposed it was found contracting 42 times in a minute. The irritability of this organ was not lost until three hours after.

Exp. 13[†]. A portion of the cranium of a rabbit was elevated in like manner, and forty drops injected on the surface of the brain. At first the animal appeared lethargic and tottered. After ten minutes it was violently convulsed, and in the space of one minute and an half more, was dead. When the thorax was opened the heart was found contracting with considerable force.

The irritability was exhausted in all the muscles subservient to voluntary motion; they were repeatedly irritated, but in vain.

In these experiments, it is clear, that opium has a very powerful and instantaneous action upon the brain, that it is diffused over the whole nervous system, evinced both by the general convulsions preceding death, and the total consumption of irritability in the voluntary muscles, and which was equally as complete as if the opium had been applied immediately to the parts themselves.

It was next examined, if when opium is introduced into some other organ, its effects are extended by the nervous system to distant parts.

Exp. 14[‡]. All the parts as near as possible to the pelvis of a frog, on both sides, were divided, leaving the ischiatic nerves uninjured. These were afterwards secluded from the air, by the divided edges of the skin being drawn together by slender threads. Three frogs were experimented upon.

Twenty drops of solution were injected into the stomach of one frog. The animal lived four hours after.

On examination after death, the irritability was destroyed in all the voluntary muscles.

Into another frog thirty drops were injected: After sixteen minutes the animal was convulsed; the extremities below the knee which had no communication with the

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[*] Vide p. 50. Exp. 31. [†] Vide p. 51. Exp. 33.

[‡] Vide Inaug. Dissert. p. 59. Exp. 38—39.

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superior part of the body, except by the undivided ischiatic nerves, were likewise affected by convulsions, and in two hours and ten minutes the animal was dead.

The ligatures which united the divided edges of the skin of the thighs were then separated, and the ischiatic nerves exposed; they were compressed; the compression of the nerve on one side, produced a slight contraction in one of the muscles in the lower part of the limb, but when repeated, no contraction followed; the compression of the nerve of the other limb, occasioned no contractions. The irritability in all the other muscles was exhausted.

Into a third frog, prepared in like manner, forty drops were injected: after fifteen minutes the animal was convulsed: after the space of two hours it was dead. Compression of the nerves did not excite the least motion in any of the muscles beneath, and when the skin was removed, the application of salt was equally as ineffectual, not the slightest degree of contraction was rendered visible.

From the event of these last related experiments we are instructed, that the effect of opium is extended to the most distant parts of the body, although the only communication which remains between the extreme parts and the body itself, is by the continuity of nerves, and these palpably not in a state best adapted to convey impressions.

It yet remained to be examined if by any other communication the effect of opium could be extended to distant parts, if the supposed integrity and indivisibility of the irritable principle was capable of doing it.

Exp. 15[*]. The spine of a frog was divided above that part from whence the nerves issue which supply the inferior extremities; care was taken not to wound any other part.

After this operation, the muscles of the inferior extremities retained their irritability, and though the animal had lost the power of voluntary motion in them, it had strength to drag them after its body.

Into the stomach of a frog thus prepared, forty drops of the solution were injected; seventeen minutes after, all the parts of the animal above the point of the division of

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[*] Vide Inaug. Dissert. p. 62. Exp. 42.

Effects of opium on the living system.

the spine, were violently convulsed, and in one hour and forty minutes the animal was dead.

The upper part of the body was then separated from the lower, at the part where the spine had been divided, and the following was the state of irritability in the different parts.

To the muscles of the breast and those of the superior extremities, salt was applied, but without exciting the least contraction or motion.

The iliac nerves below the point of the division of the spine were compressed; vivid and frequently repeated contractions were excited in all the muscles of the legs and thighs.

This experiment was repeated several times and invariably presented the same result. All the muscles of the body above the point of the division of the spine, lost the irritability; on the contrary, below the point of division, the irritability of the muscle remained unimpaired after the death of the animal, as was rendered evident both by the compression of the nerves and the application of salt.

To examine these circumstances a little more minutely, another experiment was made.

Exp. 16*. The ischiatic nerves were divided on both sides in a frog, near their exit from the pelvis; this operation does not render the limb entirely paralytic. The animal still possesses a voluntary power over the muscles of the thigh, in a considerable degree: The upper point of the leg is rendered nearly paralytic, the lower point of the leg and feet are rendered entirely paralytic.

Into the stomach of a frog, thus prepared; thirty drops of the solution were injected; after twenty-one minutes the animal was convulsed; the convulsions extended to the thighs; the legs and feet were not in the smallest degree affected by convulsions. In one hour and eighteen minutes, the animal was dead.

The application of salt to the inferior extremities, the lower joints of the legs and feet, produced rapid and frequent contractions; the muscles of the thighs at first did not contract, but after salt had been applied some time, feeble contractions were excited. The salt applied to the muscles of the superior extremities and to those of the breast and back, was incapable of exciting the smallest degree of contraction.

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* Vide Inaug. Dissert. p. 66. Exp. 44.

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In this manner I submitted the experiments and opinions of the Abbé Fontana, to an accurate investigation, and I did not draw conclusions different from his without the conviction that the experiments which I have related were carefully made and many times repeated, and in the presence of those whose bias led them to favour the opinions of the Italian physiologist. I shall therefore conclude this part of the paper, with a general enumeration of the facts which have been ascertained.

The first series of experiments proves that opium applied to the muscular fibre (the heart) exhausts or consumes, the irritability of that organ. *Vide* Exp. 1st. 2d. 3d. and 4th.

The second series of the above quoted experiments proves that the effect of opium is transmitted to distant parts of the animal body, when the agency of the circulation is both withheld and destroyed, and in as rapid a manner as when the circulation of the blood is entire and vigorous. *Vide* Exp. 7th. 8th. and 9th.

The third proves to a certain extent, that opium either does not exert any immediate action upon the blood, or that this fluid is an insufficient medium to convey it to distant parts of the system. *Vide* Exp. 10th.

The fourth series proves that the effect of opium is directly exerted upon the nervous system. Exp. 11th, 12th, 13th. That in proportion to the unity and integrity of this system, the effects of opium are extended to distant parts. Exp. 8th. and 9th That where this integrity is only partial, the effects are only partial. *Vide* Exp. 16th. That where the integrity is interrupted, the effect of opium is interrupted. Exp. 15th. And finally, that the *una et indivisa proprietas* of irritability is inadequate in any degree to extend or communicate the effects or operation of the above-mentioned power *.

VI. *On*

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* In the dissertation which has been so often quoted, the above experiments will be found supported by many others, the tendency

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VI.

On the Methods of improving Poor Soils, where Manure cannot be had. By JOHN ALDERSON, *M. D.**

Observations and enquiries respecting the improvement of poor soils.

BY soil is generally understood so much of the earth's surface as has been acted upon by the sun and air, and impregnated from time to time with the result of vegetable and animal decomposition; but, as some plants will grow where no such impregnation has taken place, we shall consider this as *mould*, and define soil, a compound of certain proportions of the simple earths, of which Naturalists reckon six or seven; and as three of these generally prevail, it will be quite sufficient for my purpose to note, that these three are, chalk, flint, and clay. With chalk and clay every person is acquainted; and the common mode in which flint affects the agriculturist, is in the form of sand.

All writers and experimentalists have agreed, that none of these earths will, separately, answer the purposes of

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of all which verge to the same point. In that, the general criterion which was established to denote the influence of opium, was founded on the observation of convulsions preceding death, and the loss of irritability in the muscular fibre after death. The quantity of this remaining was denoted by the frequency and strength of the contractions upon the application of common salt. It was, after many trials with other substances, found to be the most certain and effectual test. The manner in which salt produces this effect is no less beautiful than singular. It does not so much appear to excite a muscle to contact, because a certain portion of irritability is present, as it appears to bestow irritability, if this principle is not too much extinguished and the vitality gone. A muscle which is incapable of contracting on the application of a mechanical stimulus, and is relaxed and pale, will, on the application of salt, exhibit very frequent and strong contractions, assume gradually a beautiful florid colour, and will then become obedient to other stimuli, to which before it was insensible. Thus it will be found to be a better restorer of irritability to the muscular fibre, than muriatic acid, related by Humbolt.

* Extract from a Memoir read in the Holderness Society, and published by Harding, St. James's Street.

agriculture;

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agriculture; but that, when properly mixed, they certainly do support the roots and add to the growth of plants; and according to the best information on the subject, if taken generally, the soil, when divided into eight parts, ought to consist of the following proportions;—three parts of clay, three of chalk, and two of flint, in the form of sand; this last admitting of great variation with respect to its fineness or coarseness, according to the nature of the climate *.

Many plausible reasons have been assigned why this admixture of the earths is necessary for the purpose of forming a good soil. First, a soil consisting entirely of clay, would not part with its water sufficiently; chalk would part with it too fast, and flint would not retain it at all. Secondly, there are many of the plants we wish to cultivate, whose tender fibres are not able to penetrate clay; others that will not be sufficiently at rest from the loose and changeable nature of sand; and others that cannot act upon chalk †.

If, then, the fertility of soil depend upon the due admixture of the various earths, we may safely infer, that its sterility, or poverty, proceeds from the want of that combination. If land be barren when consisting of only one species of earth, its poverty will be in proportion the superabundance that the soil possesses of that species, let it be flint, clay, or chalk.

Experimentalists having then agreed, that a due mixture of the earths is necessary to form a fertile soil, and that barrenness proceeds from a want of the proper proportion, we see the necessity of being precise in our description of the soil we call barren.

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* These proportions will differ according to the quantity of rain that commonly falls in any given place. I need not here enter upon the reasons why more rain does fall in one place than another; the fact is indubitable, and I recommend the placing rain-gages in different parts of the country, in order that, by comparing the result with the experiments now carrying on in other countries, we may be enabled to say what it the best proportion for this district.

† See Kirwan, on Manures.

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If I am asked how to improve a certain field, I should immediately wish to ascertain what is the nature of its soil; in which kind of earth it is deficient; and in what it superabounds.

If it be all clay, its proportion of chalk and sand must be added; and, where these cannot be had, substitutes may perhaps be found: stiff clay soil is made more open, in some countries, by burning portions of it in heaps, and then ploughing the hardened earth into the land.

If the soil be sand, a frequent source of barrenness in different parts of Suffolk (where I have seen whole acres of barley blown away,) then clay becomes useful, and marl the best possible ingredient*.

An ingenious man, having obtained a grant of some waste sandy land, which, till then, had been wholly unoccupied, was allowed to enclose as much as he could cultivate. He found near the foot of a hill a stratum of clay, with which he covered, the first year, an acre of sand, and then sowed it with grass seeds; this succeeding, he followed up his plan year after year, till he formed a complete surface of grass on many acres,—which, ploughed up last year, produced him nine quarters of oats *per* acre. Thus land, which, but seven years ago, would not have maintained a single sheep, became fertile, and of considerable value †.

These premises being granted, and the facts established on the authority of many and repeated experiments; let us see, if any theory can be formed to account for the circumstance, why a mixture of the earths should be necessary for the purposes of agriculture.

The changes which take place in combustion, and those changes which constitute or exhibit animal and vegetable life, have often been compared: Food which supports fire, (as oxygen) is well known to contribute to the support of

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* Common marl contains from 66 to 80 parts of pure chalk; the remainder is in various proportions pure earth of alum and silex. *Kirwan.*

† This system, on a much larger scale, has been pursued by one of the most intelligent farmers in Suffolk, Mr. Rodwell, for which he obtained a medal from the Bath Agricultural Society.

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ife; and their products are in many instances the same (as carbon.) Now, in order to illustrate the present subject, I would carry the comparison farther than it has hitherto been done, and I would draw an inference by analogy from the process of fusion, and shew how requisite it is to make a due mixture of earths for the support of vegetable life, from the necessity there is of mixing these very earths in certain proportions, in order to render them capable of being acted upon, so as to be chemically combined, by means of fire.

If I put pure clay, chalk, or flint into a crucible, and place it in the hottest part of a furnace, no alteration or change takes place; it will indeed lose the water or air that was attached to it, but the earth will remain the same, for it is perfectly irreducible; if, however, I mix them in certain proportions, and then apply the same degree of heat, they will liquify, continue in a fluid state (so long as the fire is kept up) and their particles, intimately combined, will form a mixt mass with properties distinct from each in its simple state.

Now the operations of vegetable life resembling, as we said before, the chemical processes of combustion, may not a due mixture of these earths, when presented to the mouths or radicles of plants, render them equally capable of being absorbed, and converted by the action of the living principle into food, as they are of being fused or rendered liquid by fire? And thus am I not justified by the analogy, to draw this conclusion, that, by such an union plants derive their nourishment from the earths?—for, if the contact of these different particles of earth be alone necessary to enable the fire to produce the wonderful difference between the state of a fluid and a solid, it is difficult to be conceived, that the principle of life, so analogous to fire, should be able to exhibit similar effects, in similar circumstances; and, taking advantage of the state of the earths, when thus duly proportioned and mixed, be able to absorb and convert them into nourishment? We see also from this theory, the philosophy of ploughing, harrowing, hoeing and rolling, operations indispensably necessary to a

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good system of husbandry. Whenever plants have drawn from the soil, in the neighbourhood in which they are placed, all the materials that happen to be duly mixed, they are no longer capable of thriving, until, by a new operation, more particles are brought into contact with them. This has been sufficiently proved by persons who are in the practice of horse-hoeing, and is in effect the very object of those repeated ploughings which are performed with the view of preparing the ground for the reception of fresh seed. By this theory we see why marl becomes so admirable an addition to some soils, as to be even called a manure. Marl is formed by the deposition of clay *, and chalk from water, which, during floods and rains, has held these earths suspended, and which component parts are so intimately combined, as to be capable of being acted upon by plants. Marl I apprehend will be found in this neighbourhood at some future time, when repeated borings shall have given us the exact state of the different strata of this district †.

If I shall have the good fortune to establish this theory, we shall not have occasion to seek for the reason why chalk renders clay productive, by supposing that the latter contains an acid ‡, which the chalk absorbs, for that would be begging the question, as no such acid has been proved to exist, nor shall we have any difficulty in accounting for the different opinions of authors upon the value of lime, chalk, &c. as improvers of the soil; for, when the lime has exerted all its powers as a manure, (that is, such of it as has suffered decomposition through the medium of water, in which, till it recovers its air, it is soluble) the remainder being mere chalk, mixes with the soil, and, as it may happen, will be useful or not according to the nature of the ground it is laid upon. Lime may answer to the farmer as a stimulus, but it can only improve the soil to the land-owner, when it is laid upon clay or

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* All clay contains a portion of sand.

† Vide Kirwan's Mineralogy.

‡ See Home, Mills, and others.

sandy soil: and in this view, chalk, in an equal state of fineness, is as valuable as lime*.

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We must never forget, that plants contain a living principle, that the action of this principle seems to be analogous to the power which fire has of altering the arrangement of the particles of matter; of elevating some into the form of *gas*, and of rejecting others; and that the final cause of life, in every individual, is to bring together such particles of matter, as, when duly acted upon and assimilated, will constitute the essence of each particular living being. Thus, from the same nourishment do different living powers produce totally distinct matters, only by new arrangements: and in his laboratory the chemist, from various and different proportions of the same ingredients, can constitute and produce results, more different, in their properties and appearances, than any two species of plants or animals.

All the alterations which the earths undergo when by heat they run into fusion, become fluid, or rise into vapour, are produced by operations very similar to those of digestion and chylification in the body. Every particle of matter, by one process or another, is capable of being converted into aeriform fluids, which, in rising from the surface, meet, intimately mix and form new compounds. The same may be affirmed of composts: the intermixture of various substances produces decomposition, particles, formerly united, are separated, and new arrangements take place.

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* The nature of the lime employed must be attended to according to the nature of the soil to be improved.† Chalk, when burnt into Lime, contains from 5 to 10 per cent. of sand or clay, whereas some lime-stones contain from 50 to 80 per cent.; some also contain Magnesia, which according to Mr. Tenant, Philos. Trans. is not only not useful in agriculture, an improver of the soil, but hurtful to vegeetable life. Magnesian lime-stone may be discovered by the slowness with which it dissolves in acids; and it may be easily detected in chalk, by adding a sufficient quantity of the vitriolic acid, which, uniting with the magnesia, forms the bitter purging salt very distinguishable by its bitter taste.

† This opinion of Mr. T's, is controverted, by Mr. Headrick. See Farmer's Mag.

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All the products of nature seem destined to perpetual change and alteration; and the fibrous roots of plants appear intended by providence to produce the first stage in the transmutation of inert matter into life. Thus, by decomposition and absorption, earth becomes vegetable; vegetable matter is no sooner decomposed in the stomach of animals, than it is capable of being converted into animal matter; and when farther purified by the delicate organs of the human body, reaches the utmost perfection of created intelligence. *

Having thus generally stated the necessity of a mixture of earths, in the formation of a good soil, and pointed out the reason for that necessity, I shall beg leave to particularize a few things more in answer to the question.

It has been a commonly received opinion, that oil is the principal food of plants; but oil can no more enter the fine vessels of plants, than any one of the simple earths; it must therefore be decomposed and resolved into its elements, as well as any thing else. Oil may, and probably does, contain a very large portion of the substance which constitutes the chief food of plants in certain stages of their growth; but it must be decompounded to produce digestion, in the same manner as we have proposed in the admixture of the different earths. Alkalies and lime will render oil capable of mixing very intimately with water; and we are thence led to conclude, that they may contribute to render it more digestible, and thus capable of entering into the composition of the plants destined to be nurtured.

This doctrine may be farther illustrated by the process which milk undergoes in the stomach and bowels —Milk does not enter the lacteals of animals; but must undergo decomposition, and be digested, as well as any other food, before it can serve the purposes of nourishment.

There are however many other things to be done, before barren soils can be productive, and which may be be done where the due admixture of the earths is not to be obtained.

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* "And the Lord God formed Man of the dust of the ground *Genesis* ii. 7."

"For thou exist'st on many a thousand grains
that issue out of dust." SHAKESPEARE.

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There are various processes found adapted to particular soils, the introduction of which may reward the industry of the husbandman.

1st. Thus the wolds of this country have been enriched by the cultivation of saint-foin, and tons of hay are now produced, where one blade of grass could scarcely have been found a few years ago.

2ndly, Thistles, which are capable of deriving nourishment, and growing to a large size, where no other plant can exist: these by the exuviæ, or remains they leave, and the protection they afford to other plants and many animalculæ, tend to ameliorate the soil; but, whether they should be suffered to grow to a crop, and advantage taken of their product, or ploughed in as manure, is a question which I shall not agitate at present.

3rdly, The cultivation of spinach may be recommended as calculated to answer the same end, the prickly kind being the hardiest is to be preferred. Succulent plants impoverish the ground but little, because they derive a great part of their nourishment from the atmosphere, as may be easily proved from the alœ tribe, which will lie out of the ground for a great length of time without being hurt, drawing their nourishment from the atmosphere alone; and certainly these fleshy succulent plants, when ploughed in, will afford a very considerable supply of food for more useful plants *.

4thly, Buck-wheat also, and fumitory, a common weed upon chalky soils, may be converted to every useful purposes as a stimulus to vegetation; for the latter, when burnt, affords an uncommon quantity of the fixed alkali, so well known to be a most powerful stimulus to the growth of plants; and as the poorest soils may, by a particular management in the use of stimuli, be made productive, so an alternate crop of such plants with corn, seems to be an eligible mode of cultivating poor soils, where lime and manure are not to be had †.

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* "All succulent plants make ground fine and of a good quality." *Vide Biberg' Œconomy of Nature.*

† In the 3d Volume of the American Transactions, there is a paper on the cultivation of the eastern shore Bean, for the express purpose of being ploughed in as a manure.

5thly.

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5thly, The planting of forest trees, as tending to defend the more valuable plants from the injury they are exposed to in a poor soil, is an object well worth attention; more particularly on grass land. Some author, in the Academy of Sciences, has proved, that land exposed to a long current of wind, which blew over a large tract of barren waste, would produce nothing but poor grasses, so long as it remained thus exposed; but, when this current was broken by a few hedges and plantations of forest trees, it became capable of propagating and rearing the most useful and prolific plants. Perhaps the atmosphere attracted by the trees. parts with its electrical matter, which has been found highly conducive to the growth of plants. The agitation given to the air, when driven against the hedges and trees, may dispose it to a decomposition highly favourable to its yielding nourishment; and on this principle, I apprehend, that in districts where the air is partially obstructed by hedges and trees, it always tends more to the amelioration of land. than where stone walls and mud fence are employed.

6thly, Planting oziers, on wet land, is another mode of answering the end proposed in the question. Lands not worth half-a-crown an acre on the side of the Trent, have been planted with oziers, at the expence of four pounds per acre, and since let for four guineas an acre per annum.

7thly, One source of barrenness in soils is the presence of the calx of iron. The calx or rust of iron may be known by the redness or blueness it gives to most soils, with which it is incorporated. It may appear extraordinary to many, that this iron should be the result of vegetation, but the fact is incontrovertible*. I have reason also to believe, from observation, that some trees and plants, are more disposed than others to produce the mineral earth; and it behoves the improver of the soil to ascertain, what these plants and trees are. Of trees, the willow tribe, and alder; amongst plants, the whole order of rushes, and above all, mosses, most assuredly abound in iron, and ought never to be suffered to exist on cultivated land.

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* Vide Thompson's Chemistry, Vol. IV. page 228; Chaptal, Vol. III. page 170.

8thly,

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8thly, The action of water upon soils in general ought not to be overlooked. Lying long upon the ground, it certainly tends to the destruction of those plants we wish to cultivate.

Hence in all countries contrivances should be resorted to, to carry off the water, when its continuance would produce this effect.

But thousands of acres are barren for want of water; and there are few situations in which, if kept up in reservoirs, it may not be employed at times, with considerable advantage*.

In a variable climate, and a cultivated country, like ours, all the water that falls might be employed in agriculture. In the present state of things, perhaps, the expence might be greater than the profit; but, should engine work be so far improved, as to reduce the price of labour, and be introduced into practical husbandry, it will then, in a level country like Holderness, be no very difficult matter to place reservoirs and drains in such a manner, that a whole farm may be either drained or flooded at pleasure.

The Chinese, who certainly possess the best cultivated country in the world, are not content to make canals for the purposes of trade, they dig many others to catch the rain, with which they water their fields in time of drought; during the whole summer, the country people are busied in raising this water into ditches, which are made across the fields; in other places they contrive large reservoirs, made of turf, whose bottom is raised above the level of the ground about it; and, if they meet with a spring of water, it is worth while to observe how carefully they husband it; they sustain it by banks in the highest places, they turn it a hundred different ways, they divide it by drawing it by degrees, according as every one has occasion for it, in so much that a small rivulet, well managed, sometimes carries fertility to a whole province †.

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* With a double view, *catch-water drains*, as they are called, ought to be formed, not only to prevent the low lands being flooded during violent rains; but, by keeping up the water, to preserve it to be employed, at a proper season, in irrigation.

† Vide Le Compt's Letters on China.

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Considerable expence has been incurred in this country, in order to find the best means of carrying off the water; but sufficient attention has no where been paid to the improvement of the soil, by the introduction of water for irrigation *.

Great advantages, of late years, have been derived from *warping*, along the banks of the Ouse, the Trent, and the Dutch river, where the water is let in at the flood tide, and suffered to rest, and deposit its mud, until the ebb. By this process, repeated twice a day during five or six summers, a new soil is formed, to the height of six feet, which, in the following spring will be firm enough to receive seeds, and in summer to carry an ox. Thus land, which was before only a peat bog, comparatively worth nothing, may be let for forty-five shillings per acre.

The Dutch river affords the best warp, because it nearly empties its whole channel during the ebb, and consequently contributes only the tide, there being very little back water during the flooding season; and hence, too, dry summers are better than wet ones; for, when the freshes are out, the water, though muddy, contains nothing but clay, washed from the tops of mountains, and the banks of rivers; but the muddy water of the tide contains all the products of the Humber, which consist of a large quantity of animal matter, as well as various species of earths.

An enterprising and spirited individual has proposed to warp the whole of several parishes extending over many thousand acres of bog, for one sixth of the land gained; which he purposes to effect by cutting a general canal, through these parishes from the Dutch river into the Trent †.

9thly,

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* The whole of the low lands of Anlaby and Hessle might have been watered at pleasure, by keeping up the spring that passes through Anlaby town; or, by boring and piping the springs that may any where be found, and which will rise, in most places within those parishes, above the surface.

† The farmer, the land-owner, and the public, have all been benefited by this improvement. The farmer, by his industry and attention, has converted the most barren bog into land capable of bearing the plough, and of feeding an ox. The landlord by only foregoing the rent during the time the land is under water, has been able in a few years to increase the value of his property fifty fold.

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9thly, Those whose lands border on the Humber, or the sea, may derive a further advantage from this vicinity, than what arises from mere irrigation. I have already taken some pains to point out the absolute necessity there is for bringing the different earths into close union, in order to procure that decomposition, necessary to their being converted into vegetable life; the same doctrine is applicable to composts, and may now be extended to salt water.

Salt water consists of certain alkaline salts united to the marine acid, which form a neutral, not easily decompounded in common earth, and therefore not a very active manure. To obtain the greatest possible advantage from sea water, it ought to be decomposed, which may, in part be effected by adding to it gypsum, alabaster, or plaster of Paris—a matter compounded of lime and the vitriolic acid. When this is well soaked in sea water, the vitriolic acid will in time quit the lime, seize the alkaline basis of sea-salt, and leave the marine acid to combine with the lime; * but in all these operations a large quantity of earth or soil should be compounded with the result, before it be applied as a manure, the salts being of themselves too pungent, if applied to vegetation unmixed with earth. This method ought also to be pursued, when any composts are formed.

10thly, Sand is also capable of further use than what is merely pointed out by the foregoing theory. In Norfolk it is thrown into the yards and stables, to absorb all the moisture; and the horses and cattle that are fed in the stalls, with cut grass or vetches, are bedded with it, in order that their urine may be absorbed and employed for the future amelioration of the soil.

11thly, The banks along the old or natural drains, which

The country at large not only obtains an increase in the supply of the food of man; but thousands of acres of the most noxious fens, prolific only in agues and remittent fevers, are by this process, covered with a healthful appearance.

* According to Berthollet, chalk is capable of decomposing sea salt, in the course of four years, and by that process, the natron of alkali is suffered to chrystalize in the lakes in Egypt*.

* Vide Memoirs on Egypt.

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have been formed by the overflowing of particular tides, when the Humber was not so well restrained within its limits as at present, are capable of being employed, to improve the soil. It is well known, that where the water first begins to deposit, there the best soil is produced; and, as these banks have been formed by repeated depositions of this kind, they consist, several feet deep, of valuable earth, which may be led away, and employed as a manure.

APPENDIX.

IN answer to those gentlemen in this Society, Mr. President, who have said, there is no land in Holderness bad enough to grow thistles upon, I ask, is there no land that requires occasional fallowing? If this be allowed, then the question will be, whether the cultivation of thistles be, or be not, more advantageous to the land, or productive to the farmer, than letting it lie fallow. Now it having been stated by such authority as Dr. Withering, that* thistles grow and flourish upon clays, where no other plant can exist without manure, and that, where they have grown, other plants may afterwards be propagated, will not a crop of thistles be found highly advantageous to the farmer? † For if they exist upon land, and draw none

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"* Thistles, as the most useful, are armed and guarded by nature herself. Suppose there was a heap of clay, on which, for many years no plant had ever sprung up, let the seeds of the thistles blow there, and grow, the thistles, by their leaves, attract the moisture out of the air, send it into the clay by means of their roots, will thrive themselves, and afford a shade. Let now other plants come, and they will soon cover the ground." Biberg's Œconomy of Nature, translated by Stillingfleet. See also Withering's Botanical Arrangement.

† "It is probable," says Parkinson, "that the sow-thistle, were it properly cultivated, would become one of the most fattening plants the earth produces. Sheep, when in clovers, &c: will feed upon it so greedily, as to eat the very roots; pigs likewise prefer it to almost any other green-food; rabbits will breed more speedily when fed with sow thistles, than with any other food I know

none of the common nourishment from it, will not they, in a state of decomposition, be a valuable addition to nourishment, or at least prove a powerful stimulus to more valuable plants, which we may afterwards wish to cultivate upon the same land.

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It is not the chief end of the existence of plants, to bring dead earthy matter into a state of life? We know, that when there is a due mixture of the earths, any plant we wish to cultivate will thrive and produce this effect; and that, if we add a sufficient stimulus, or manure, then such plants will yield the largest increase; or even where there is not a due mixture, provided we can supply a large and repeated quantity of the stimulus that even there, they may for a season, be induced to make vigorous shoots and even perfect themselves. But in the case of barren soils, where this due mixture is not present, and where (as the question implies) the stimulus is not to be had, it is the object of our enquiries to find out a plant that will grow, and either yield an immediate profit, or, by improving the soil, enable others more valuable to succeed in future. Now, as the soil immediately referred to is confessedly clay, and as thistles will grow on it, and leave behind them such a quantity of refuse as will enable other plants to succeed, ought we not to recommend the cultivation of them on poor land, with the expectation that they will add more to the soil than they take from it, and so become improvers.

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know of, except dandelion, which is of the same nature, and is now sold in Covent garden market, to the breeders of tame rabbits. A man of my acquaintance, who was allowed better skill with stallions, than the generality of mankind, used to search for sow-thistles to give to his horse. We have a well-known and decisive proof of the nutricious properties of sow-thistles, in the fat wether sheep, fed to an amazing size by Mr. Trimpel, of Bicker Fen, near Boston, Lincolnshire, upon the fen land. This Sheep was bred by Mr. Hutchinson, in Hail fen, from a ram bred by Mr. Robinson, of Kirby, near Sleaford. He never ate any corn, oil cakes, &c. but fed wholly upon grass and herbage; being turned with many other sheep into a field of clover, this sheep was at first observed to search for sow-thistles, and would eat no other food whilst any of them could be found in the part of the field

Observations and enquiries respecting the improvement of poor soils.

It is an old farming maxim, that plants of the same species will not thrive successively on the same land, for, where one plant has died, another of the same species cannot live. This is the case with animal life, and with combustion, or fire; two processes extremely analogous to vegetation *.

Where a candle becomes extinct (provided no fresh air be admitted) another cannot be lighted, and where an animal has died, another of the same species cannot exist; but other combustible matters may be made to burn, and animals of a different species may, for a time, be made to live. Thus I would infer, that, where a crop of wheat has grown, been brought to perfection, and died, there another crop of the same kind will not succeed; although different kind of corn, pulse, or grass may.

Now the reason why the former phenomena take place, is partly by the abstraction or taking away of something from the air, necessary to the life or support of the first animal or combustible substance, and partly, by the giving out something, which, (though inimical to the individual that parts with it) is nevertheless, (so far from being hurtful to others) the very matter some kinds prefer to live upon. The same or similar processes may go on with plants; where a crop of wheat has grown, the materials for the sustenance of wheat may not only be absorbed, but the situation in which it has lived and died, may be so impregnated with its excretions †, as to be inimical to the life

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field, that was hurdled off successively a little at a time; when the field was bare of thistles, his attendants gave him three times a day, from two to five pounds at a meal. This sheep, when killed, measured five feet from the nose to the tail, the rump or cushion eight and a half in depth, plate or fore flank of the same thickness, breast end seven inches, one yard five inches and a half round the collar, and weighed sixty seven pounds a quarter, avoirdupoise weight; the legs were estimated at forty pounds each, but if cut haunch of venison fashion, would have weighed fifty pounds each; for which the proprietor, Mr. Lumby, was offered *two shillings* a pound; so that the legs only would have brought ten pounds. *Vide Parkinson's Experienced Farmer.*

* Vide preceding Observations.

† This theory may be exemplified by a fact, which I have fre-

Observations and enquiries respecting the improvement of poor soils.

life of any future plant of the same species, although, as we said before, not for any other kind, until, by a proper succession, this very matter be attracted and absorbed into the substance of other plants; and thus we are enabled to point out the obvious principles that govern those rotations, which the experience of all ages teaches.

The great desideratum, or object of our enquiries, then will be, what are the best means of bringing together a fresh set of materials remaining in the soil? And, what is the succession best calculated to remove from the land, the dregs of former crops; or, what plants will best live and thrive where others have previously been cultivated? I know it is the general opinion of men of experience, in this part of the country, that fallowing can alone effect the former, and it is the general practice to make the black and white corn succeed each other, in order to effect the latter. Let us, however, enquire a little further. I am aware, that the fact (to account for which, I have ventured to frame a theory) has been denied by authority*, long celebrated in agriculture; I mean, "that wheat cannot be made to grow upon the same land, for two or three years successively;" and we are referred to an experiment made in a field belonging to Mr. Barlow, near York, for the proof of the contrary; but what does the experiment say? It says, that "plants of wheat were taken from a situation, in which they had stood the winter, and transplanted into a field that had grown potatoes; had been afterwards ploughed, harrowed, and rolled, and were pricked down an inch deep, and nine inches from each other;" and, "that it is proposed to do the same for several successive years, in order to determine the doubtful point, whether wheat can be raised for a number of successive years, upon the same land;" and, "that, instead of letting the land lie waste, under a summer fallow, it may be made to produce a crop of cabbages, turnips, pease, beans, potatoes, or summer vetches, as preparatory to its being planted with wheat.—Can this experiment militate in

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frequently observed; the fibrous roots of thorn, and many other trees and plants, where they enter chalk or clay, leave behind them an ochre, or irony mark.

* Vide Dr. Hunter's Circular Letter.

Observations and enquiries respecting the improvement of poor soils.

the least against the doctrine here advanced? or does not rather go to prove the truth of it: for, is it not clear, that, in order to succeed, it is necessary by transplantation, to remove the plants from a soil, out of which they have already extracted a certain portion of its nutricious matter, and in which they have already deposited something which might be hurtful, when they came to flower and seed? *

That the plan of transplanting wheat will answer I have not a doubt; it has long been practised by several gentlemen in Norfolk; and upon the principles here laid down and agreed upon, we can judge how it may prevent the necessity of fallowing, as it goes to prove what I have before hinted at in my theory, that, by sowing a summer crop of leguminous plants, such as pease, beans, vetches, &c. or the useful roots, turnips and potatoes, every thing hurtful to the growth of wheat may be taken from the ground. This, however, will be perhaps as profitably done by substituting a succession of other white corn, instead of wheat, in a regular manner.

From the foregoing, then we are led to conclude, that, by attention to a proper mixture of the earths, in order to bring various particles into intimate union, by frequent new combinations, and by a succession of plants dissimilar in their habits from each other, we may so far improve agriculture, as to have yearly crops from such a soil as ours; and that it will be possible, in time, to bring every acre of ground in this district into an almost equal degree of value †.

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* Some warp land on the other side of the Humber produced wheat for seven years, successively, without manure; but, this only proves the possibility, that earth may be accidentally so well arranged and mixed, as to afford nourishment for a long succession of crops.

† There is a practice, frequent in Holderness, which deserves to be reprobated, and that is, suffering stubbles to lie unploughed after harvest. It appears to me a shocking waste of the valuable soil, to suffer it to be exhausted at the latter spring, in producing useless plants and weeds. The great object of agriculture, is to take advantage of every circumstance, that can oblige the earth to produce only the profitable parts of the vegetable creation; to suffer the land, therefore, to support what at present we know not the use of, is in the highest degree injurious and impolitic.

VII.

Description, of an Apparatus for transferring Gasses over Water or Mercury, &c. *By the* REV. GILBERT AUSTIN, M. R. I. A.*

Difficulties of transferring gasses by the usual apparatus.

THE difficulty of transferring gasses from one jar or receiver to another without loss, or mixture of atmospheric-air, by the common mode in the pneumatic apparatus, must have been experienced often by philosophical chemists. And this difficulty is encreased when very large jars are used, and when the production of gas in them is inconsiderable; as when oxygen gas is obtained from vegetables exposed to light, or from the decomposition of water. Of the small quantity, obtained in this manner, a portion is often lost in transferring it into a smaller jar for the purpose of subjecting it to examination; and the result of the experiment is rendered uncertain, if the object be to measure the quantity. In order to obviate this inconvenience, I beg leave to submit to the Royal Irish Academy the description of a small apparatus, which I have found to answer well, and conceive may be admitted as a useful instrument into a philosophical laboratory.

A new apparatus in which these glass plates are applied to the mouth of the jar. &c.

The principal part of this apparatus consists of two pieces of plate glass, with a hole of about half an inch diameter drilled through each. They should be something broader, and about twice as long as the diameter of the jars used in collecting and transferring the gasses. The holes should be disposed as in the figure. That in the plate (Fig. 1.), marked *(a)* should be nearly in the middle of the piece. The hole in the upper plate *(b)* near the extreme edge. The upper plate is shorter than the under plate, and its edge is grounded fair and straight, so as to fit the edge of the *third* plate, which is not drilled, and should be a square piece cut off the second plate, as it is very necessary that these two plates should be of the same thickness. The length of these plates together should exceed that of the under plate about an inch. It is rather better to grind the polish off the plates with a little fine emery, as they slide more equably over each other when so prepared. All the jars to be used with them should have their mouths ground on a flat plate with fine emery. Things being thus prepared, the transferring plates may be used in the following manner, particularly when the jars for collecting the gasses are large.

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* From the Irish Memoirs, 1806.

Improved apparatus for transferring the gasses.

When the jars, inverted in the usual manner in the pneumatic trough, are filled with the gas in any proportion, the two plates (*a* and *b*) are laid over each other in such a situation, that their holes shall not coincide; they are then plunged into the water, and the plate (*q*) applied to the mouth of the jar, and that and the plate (*b*) being moderately pressed against the mouth, so that they shall not slip, or suffer any gas to escape, the jar together with the plates, is lifted out of the water, and set with the mouth turned up. In this position the jar is ready for yielding the gas to the jar into which it is to be transferred. This last jar is now to be filled with water, taking care not to leave any air in it, and its mouth is to be closed by the third plate. It is then to be turned with its mouth downwards, and, together with the third plate on which it stands, is to be placed on that part of the under plate which is not covered by the upper plate. The edges of the third and upper plate are placed as nearly as possible in contact; and across them the small jar, filled with water, is to be slided till it rests entirely on the upper plate. The hole in the upper plate is to be filled with a few drops of water, and the jar is to be slided so as to stand over it. The upper plate, and the jar standing upon it, are then to be so moved over the under plate, that the holes in each shall coincide. The water in the upper jar, as soon as the communication is thus opened, will descend into the lower or magazine jar, and be supplied with an equal bulk of gas from below at pleasure. When a sufficient quantity is transferred thus into the upper jar, it is pushed together with its plate, in such a manner that the holes shall no longer coincide, and, consequently, the communication shall be cut off. The upper jar is slided back upon the third plate, and, Improved apparatus for transferring the gasses.

and, together with the plate, is removed in the same manner as it was applied. The mouth of the jar is turned upwards, the plate removed, and the gas submitted to examination: or, with mouth downwards, the small jar is placed on the shelf of the pneumatic trough, as the experiment may require. This detail appears tedious, but the practice is very easy. In this process there is, however, some danger of disturbing the lower plate, by lifting it from the mouth of the magazine jar, and so vitiating the gas by the introduction of common air. To prevent this inconvenience, it is necessary to secure the two perforated plates to the mouth of the jar, and to each other, allowing the upper plate, at the same time, to slide freely over the other. For this purpose, it is necessary to fix the plates, and the magazine jar, in a frame; which renders the use of them very convenient, and not liable to accidental disturbance.

The two plates *(a* and *b)*, as in Fig. 1,, are fixed in the upper part of the frame: *(a)* is fastened *(b)* slides easily over it. The jar *(d)* is pressed up against the plate *(a)* by a moveable bottom *(k)*, tightened by wedges or screws. The jar may be filled with water before it is fixed in the frame, and inverted in the trough; or the air may be generated in the jar, without the frame, and then, the frame being inverted; and the plates sunk in the water, the jar may be slipped into its place, and fixed there, which is the better way. The frame and jar are then set upright, and the gas may be transferred as before, without danger or loss, or mixture.

By means of this apparatus, jars of any size may be used as magazines, with the inconvenience of being obliged to invert them in large troughs.

This apparatus, also, on a smaller scale, may be used in operating with those gasses which can only be confined over mercury. The joints of the transferring plates retain very securely any quantity of mercury, provided the height of the jar is inconsiderable, not more than three or four inches, for reasons well known to experimental philosophers. And small jars, with ground mouths, hold mercury very well, when standing, without agitation, with their mouths downwards, on ground plates of glass. The careful operator will, however, Improved apparatus for transferring the gases.

however, gently press them to prevent accidents. This apparatus may be so far reduced in size, that, on a small scale, all operations, on gasses only to be confined over mercury, may be performed with about four or five pounds of mercury: which may, in many cases, be an object of attention to the philosophical chemist.

Fig. 1. *(a)* The under plate; the dotted line marks the circumference of the mouth of the magazine jar.

(b) The upper plate.

(c) The third plate; the dots mark the circumference of the mouth of the small jar. The small dark circle shews the place of the holes.

Fig. 1. *(a b c)* The section of the plates, (as in Fig. 1.)

(d) The magazine jar.

(e) The small jar.

(f) The dotted jar shews how the small jar is placed, together with the third plate *(e)*, before it is slided across the edges *(g)* of that and the upper plate.

Fig. 3. *(a b c)* The plates as before, but fixed in.

(h) The frame.

(d) The lower or magazine jar, (as wedged up) against the under plate, by

(k) The moveable bottom.

(e) The small jar to be filled with gas from the lower jar.

Fig. 4, *A small Apparatus for operating with Mercury.*

(a b c) The plates as before.

(d) The small jar, four inches high, with a broad rim, by which the lower plate may be confined to its mouth, together with a frame in which the upper and third plates may slide. This frame may be made of hard wood, of ivory, or of iron.

(g) A section of a wooden box, to hold as much mercury as will cover the plates and frame, and admit the bent tube of

(l)

(l) A small retort or vial, with a bent tube, for generating the gas which passes through the hole of the plates. Improved apparatus for transferring the gases.

(m) A small spirit lamp.

(n) A tube, fixed so in the box, that the mercury, descending from (d) as the gas is generated, shall overflow, and be received in a cup; with which small jars may be filled for transferring.

VIII.

Description of the Mineral Bason in the Counties of Monmouth, Glamorgan, Brecon, Carmarthen, and Pembroke. By Mr. EDWARD MARTIN.*

THE irregular oval line, delineated on the annexed map † shows nearly the inner edge of a limestone bason, in which all the strata of coal and iron ore (commonly called Iron Stone) in South Wales are deposited; the length of this bason is upwards of 100 miles, and the average breadth in the counties of Monmouth, Glamorgan, Carmarthen, and part of Brecon, is from 18 to 20 miles, and in Pembrokeshire only from 3 to 5 miles. Mineral bason in South Wales.

2. On the north side of a line, that may be drawn in an east and west direction, ranging nearly through the middle of this bason, all the strata rise gradually northward; and on

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* Phil. Trans. 1806.

† The outline on the map given in the Transactions (but not copied in our Journal) begins from the N. E. corner of St. Bride's Bay, and proceeds by Haverford West, across a small part of Carmarthen Bay whence it passes near Kidwelly more northerly till about three miles south of Llandillo. From this part it inclines more southerly towards Abergavenny, but within five miles of that town it rounds to the south through Pontypool and thence to the S. W. (rounding) through Llantrissent, but whence it arrives at the coast of Swansea Bay it spreads nearly in a line to Tenby, and thence to the middle of the Western shore of St. Bride's Bay.

Mineral bason in South Wales.

the south side of this line they rise southward, till they come to the surface, except at the east end, which is in the vicinity of Pontypool, where they rise eastward.

3. The depths from the surface to the various strata of coal and iron ore depend upon their respective local situations.

4. The deepest part of the bason is between Neath, in Glamorganshire, and Llanelly, in Carmarthenshire; the uppermost stratum of coal here does not extend a mile in a north and south direction, and not many miles in an east and west direction, and its utmost depth is not above 50 or 60 fathoms.

5. The next stratum of coal, and those likewise beneath it, lie deeper and expand still longer and wider, and the lowest which are attended by parallel strata of iron ore, of which they are in some situations about 16 accompanied by irregular balls or lumps of iron ore, occupy the whole space between Llanmaddock Hill, near the the entrance of Burry river, to Llanbidie, from the Mumbles to Cribbath, from Newton Down to Penderryn, from Castle Coch to Castle Morlais, and from Risca to Llangattock, and in length of the south side of the bason from Pontypool and through Risca, Tinkwood, Llantrissent, Margam, Swansea Bay, and Cline Wood, to Llanmaddock Hill, and on the north side through Blaenafon, Ebbw, Sirhowy, Merthyr, Aberdare, Aberpergwm, Glyntowy, Llandibie, and the Great Mountain, to Pembrey Hill, near Llanelly in Carmarthenshire, and their depths are at the center range of strata from 6 to 700 fathoms.

6. The strata of coal and iron ore running from Pembry Hill, through Carmarthen Bay and Pembrokeshire, to St. Bride's Bay, are only a continuation of those in the counties of Glamorgan and Carmarthen, which lie next to and parallel with the north side of the bason, all the remaining strata rising southward; and the middle ranges on the north side of the bason, are lost between where they meet the sea near Llanmaddock Hill and the south side of Pembrey Hill, in their course towards Pembrokeshire, in consequence of a contraction of the sides of the mineral bason, or rather by its becoming shallower; for in Pembrokeshire none of the strata of coal or iron ore lie above 80 or 100 fathoms deep, consequently all those which do not lie above 5 or 600 fathoms in

Mineral bason in South Wales

in Glamorganshire and Carmarthenshire have not reached this county, by reason of the bason not being of sufficient depth and width to hold them.

7. The strata of coal at the east end of the bason running from Pontypool to Blaenafon and Clydach and on the north-side from thence to Nanty Glo, Ebbw, Beaufort, Sirhowy, Tredegar, Remney, Dowlais, Penderryn, Plymouth, Cyfarthfa, Abernant, Aberdare and Hurwain Furnaces and Iron Works, are of a cokeing quality, and from thence the whole strata of coal to St. Bride's Bay alter in their quality, to what is called Stone Coal, (the large of which has hitherto been used for the purposes of drying malt and hops, and the small, which is called Culm, for burning of limestone) ; the several strata of coal from Pontypool. on the south side of the bason, through Risca, Llantrissent, Margam, and Cline Wood, to Burry River, Llanelly, and the south side of Pembrey Hill, are principally of a bituminous or binding quality.

8. Notwithstanding the principal strata of coal in Glamorganshire, lie from 5 fathoms to 6 or 700 fathoms deep, still it has not been necessary to pursue these strata deeper than about 80 fathoms.

9. The veins of coal and iron ore, in the vicinity of most of the iron works in Monmouthshire and Glamorganshire are drained and worked by levels or horizontal drifts, which opportunity is given by the deep valleys which generally run in a north and south direction, intersecting the range of coal and iron ore, which run in an east and west direction, under the high mountains, and thereby serving as main drains, so that the collier or miner here gets at the treasures of the earth, without going to the expence and labour of sinking deep pits, and erecting powerful fire-engines. However, in process of time, in situations where the coal and iron ore that are above the level of these natural drains, become exhausted, it will be found necessary to sink shallow pits, and erect fire-engines for the draining and working of the coal and iron ore, and at a future period, pits of greater depths, must be sunk for the same purposes.

10. There are 12 veins or strata of coal in this mineral depository, from 3 feet to 9 feet thick each, which together make 70½ feet: and there are 11 more, from 18 inches to 3

Mineral Bason in South Wales.

feet, which make 24¼ feet, making in all 95 feet; besides a number of smaller veins from 12 to 18 inches, and from 6 to 12 inches in thickness, not calculated upon.

11. By taking the average length and breadth of the foregoing different strata of coal, the amount is about 1000 square miles, containing 95 feet of coal in 23 distinct strata, which will produce in the common way of working 100,000 tons *per* acre, or 64,000,000 tons *per* square mile.

12. If the whole extent of this mineral country was an even plain, the border or outbreak of each stratum would appear regular and true; but owing to the interposition of hills, valleys, the edges of the strata, if nicely measured and planned, would seem indented and uneven, yet in many instances the due range is totally thrown out of course, in consequence of knots, dikes, or faults.

13. These faults or irregularities are not confined to the edges of the strata, but they take grand ranges, through the interior of the bason, generally in a north and south direction, and often throw the whole of the strata, for hundreds of acres together, 40, 60, 80, or 100 fathoms, up or down, and still there is seldom any superficial appearance, that indicates a disjunction, for the largest faults frequently lie under even surfaces.

14. As every stratum rises regularly from its base to the surface, and frequently visible and bare, in precipices and deep dingles, and often discovered where the earth or soil is shallow in trenching, or in forming high roads, and by reason of the whole of the country within this boundary being so perforated by pits, and so intersected by the various operations of art and nature, it is not probable that any vein of coal, iron ore, or other stratum remains undiscovered in this mineral bason.

15. Glamorganshire engrosses far the greatest portion of coal and iron ore, Monmouthshire the next in point of quantity, Carmarthenshire the next, Pembrokeshire the next, and Brecknockshire possesses the least.

16. The strata of coal and iron ore in the last named county, which are the lowest in the bason, break out northward, and only take place in the three following distinct spots, *viz.* 1st. From Turch River (which is the boundary between Lord CAWDOR and CHARLES MORGAN Esq.) across the river Tawe and

the Drin Mountain to the great forest of Brecon. 2d. A corner of ground from Blaen Romney to the north of Brynoer. 3d. Another spot, from Rhyd Ebbw and Beaufort Iron Works, through Llwyn y Pwll, near Tavern Maed Sur, to where it joins Lord Abergavenny's mineral property.

Mineral bason in SouthWales.

17. *Note.* A principal fault is observable at Cribbath where the beds or strata of the limestone stand erect: another, of considerable magnitude, lies between Ystradvellte, and Penderryn, where all the strata on the north side of the bason are moved many hundred yards southward (as at Dinas.)

18. *Note.* The limestone appears to the surface all along the boundary line in the counties of Monmouth, Glamorgan, Carmarthen, Brecon, and no doubt can be entertained of its due range from Newton across Swansey Bay to the Mumbles, and from Llanmaddock Hill across Carmarthen Bay to Tenby. In Pembrokeshire it appears to the surface on the south side of the bason, at Tenby, Ivy Tower, Cockelard, Bit, Church-Williamston, Lawrinny, Cord, Canta, and Johnston, and on the north side of the bason, at Templeton, Picton, Harriston and Persfield; yet it certainly forms an underground connection from point to point.

IX.

On the Water Pits of the Glaciers of Chamouny. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Cork, 13*th April*, 1807.

I JUST now was looking over a paper from Count Rumford, in your Journal, on a curious phenomenon on the "Glaciers of "Chamouny," with respect to a pit which he observes was formed in the ice. The manner in which he accounts for it, is, I think, inconsistent with his own rules. The manner in which I would account for this phenomenon, is, that as cold water

Observations on a fact observed by Count Rumford.

lying on ice melts it sooner than warm water, so for the same reason the water which lay on the top of these pits was, as he observes, warmed by the wind which passed over them, and that which was in the bottom of the pit cold, of course it had a tendency to melt downwards rather than at the sides. Now, Sir, if you will have the goodness to publish this in your Journal, so that Count Rumford may see it, you will oblige me in hopes that I may see what he thinks of my account of this phenomenon.

I am,
Sir, &c. &c.
A constant Reader.

X.

Remark by M. DE LALANDE *on the Distance of the Stars.*

Distance of the stars.

DURING the last century it has been believed, that the annual parallax of the stars, that is the difference of their situations in the course of six months, relative to the position of the earth, does not vary a single second; whence it results that their distance must exceed seven millions of millions of leagues.

M. Piazzi, at Palermo, and M. Callandrelli, at Rome, have recently made observations on several of the stars, from which it appears that some of the stars give a difference of five seconds, particularly Lyra, which, next to Sirius, is the most brilliant star in our hemisphere, from whence it results that it is one of the least distant. If there be five seconds of simple parallax, the distance ought to be fourteen hundred thousand millions of leagues, that is to say, five times less than was previously supposed. But these observations are not yet sufficiently numerous and complete, to afford a perfectly certain conclusion.

XI.

Observations on the Soda, Magnesia, and Lime, contained in the Water of the Ocean; shewing that they operate advantageously there by neutralizing Acids, and among others the Septic Acid, and that Sea-Water may be rendered fit for washing Clothes without the Aid of Soap. By SAMUEL L. MITCHILL, *of New York.*[*]

Observations and facts respecting the component parts contained in sea water, and the useful applications of that fluid.

ANY attempts have been made to render the water of the ocean fit for the purposes of *drinking* and *cooking*, and some of these have been attended with flattering prospects of utility. By a cheap and easy process, water tolerably fresh may be distilled from common salt water, so as to help materially in a case of scarcity or want, on board a ship of good equipment. The names of Hales, Lind and Irvine, are remembered to their honour, for their exertions in this work.

To furnish needy men with the means of *eating* and *drinking*, is certainly a noble discovery. But there is another operation scarcely less necessary to the preservation of health than eating and drinking, and that is *washing* as applied to the human body, and more particularly to the clothing which it befouls. In a communication to professor Duncan, which has been published in the Edinburgh Annals of Medicine for 1799, and in the third volume of the New York Medical Repository, I have endeavoured to state the facts in detail concerning the matters secreted from the skin and wiped off by the clothes, and to shew how some of these became unwholesome, or infectious and pestilential, as they grew nasty. It was there stated that soaps and *alkalies* would render foul clothing clean, and both prevent and destroy animal poison if it was engendering there. And in a letter I wrote to Timothy Pickering, late Secretary of State to the American Government, in November

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[*] American Transactions, vol. v. The Doctor uses the term septic for azotic or nitric.

Observations and facts respecting the component parts contained in sea water, and the useful applications of that fluid.

1799, I recommended barilla or soda as a substance by which the salt-water of the ocean could be so softened and altered in its qualities as to become fit for washing the clothes of seamen.

A sea-vessel is peculiarly fitted for concentering foul and corrupting things, and for converting them into pestilence and poison. This is one of the most common accidents in sailing to the latitudes where there is heat enough to promote corruption and to exalt septic substances into vapour.

One of the most disgusting sights during a voyage is the personal nastiness of many of the crew. It is pretended that much of this is necessarily connected with the service, that the work is dirty, and especially that fresh water cannot be spared from the vessel's stores to wash the company's clothing; that soap cannot be used with ocean-water, that salt-water alone will not get them clean, and that therefore they are under a necessity of being uncomfortably nasty on long voyages, especially toward the latter part of them. Now, nastiness of a man's person and garments is necessarily connected with a similar condition of his bed, bedding, hammock and berth, and most commonly of every thing he handles or has ought to do with. If a seaman has strength of constitution to keep about and do duty, his feelings are nevertheless very uncomfortable, he is thereby predisposed to disease and in danger every moment of becoming sick; and if this should really happen, his chance of recovery is exceedingly lessened by the filth with which every thing that touches him is impregnated, and the venom into which that filth is incessantly changing.

Thus, the great difficulties with which a seaman has to struggle, are 1st, the unfitness of ocean-water to wash with; and 2d, the inutility of soap to aid that fluid in cleansing his clothes. If these can be surmounted, he will have no excuse for his uncleanness. If after this he becomes uncomfortable or sickly from that cause, it will be owing to his own laziness or negligence.

Few subjects have been discussed with more solicitude than the one, How did the ocean acquire its saltness? Whether that mass of waters derived its briny quality gradually by dissolving strata of salt, or whether it was furnished by its Creator with a due quantity of that material from the beginning,

Observations and facts respecting the component parts contained in sea water, and the useful applications of that fluid.

are questions not necessary now to be answered. It is sufficient to observe that it is kept sweet and guarded against offensiveness and corruption by the great quantity of *alkaline* matter it contains. The ocean may indeed be considered as containing some portion of every thing which water is capable of containing or dissolving, and its water is therefore found to furnish different results on analysis, when taken up from different depths and in different latitudes.

Yet various as the composition of ocean-water is, it always contains *soda*, *magnesia* and *lime*, in quantity considerable enough to be easily detected. Of these *soda* is the most abundant. *Magnesia* is next in quantity. And *lime*, though plentiful, is believed to exist in smaller proportion than either.

The *alkaline matter* so plentifully dispersed through the water of the ocean, exerts its customary neutralizing power after the same manner and according to the same laws which govern its several kinds on the land and in other places.

The acids commonly present in ocean-water are the *sulphuric*, the *septic* and the *muriatic*. The former of these exists apparently in small quantity, and is only mentioned because in some experiments it has been said to have been obtained from it in the form of a sulphate of lime, though according to the law of attractions, we might expect to find in it sulphate of soda. The vast amount of animal matter existing in the sea, would lead one à priori to a persuasion that in certain cases, particularly along marshes and shores were the stagnating water was much heated, putrefaction would engender *septic* acid, and this would in some measure mingle with the water in its vicinity, and not fly away wholly in vapour. The quantity of this acid is so considerable in some coves and bays where salt works have been established, that a quantity of it adheres to the muriate of soda or common salt and vitiates its quality. And this happens in some situations to so high a degree, that Neumman (Chemical Works by Lewis, p. 392,) takes notice of it, observing " that sea water often contains a *nitrous* matter, the *acid spirit distilled from sea salt proving a menstruum for gold*, which the marine acid by itself never does, and which nothing but

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the nitrous will enable it to do. Though however this is frequently the case, it is not always: I have examined marine salt whose acid had no action upon gold."—As to the *muriatic* acid, whether it is as some of the older chemists suppose a modification of the sulphuric and the nitrous, or as certain of the moderns believe, but a compound basis of sulphuric and hydrogene, there is evidence enough of its existence in the ocean in very great plenty.—On the whole, it may be concluded that sea-water *always* contains *muriatic* acid, *frequently septic* and *sometimes sulphuric.*

There are thus three predominating *alkalies* and as many *acids* in the ocean; and by the intervention of water they are liquefied and put in a condition to act each upon the other. Consequently the soda in the first place, as the stronger alkali attaches and neutralizes the acids in the order of chemical affinity, and forms sulphate, septate and muriate of soda. But as the *two* former are comparatively rare or scarce, the latter is the predominating compound. When there is any acid in the water beyond the capacity of the soda existing there to neutralize, that part is attracted by the *two* earths, and acording to the force of their respective combinations, forms sulphates, septates and muriates of lime and magnesia. These salts with earthy bases, in which the muriatic acid is by far more abundant than the other two acids, constitute the *bittern* and *scratch* or *slack* of the salt makers. These salted earths attract water so strongly that it is difficult or impossible to make them crystallize; but wherever they are they keep up a dampness and refuse to dry.

When chemists speak of sea salt they wish to be understood as meaning "the pure muriate of soda." This neutral compound however in its *pure* state is a great rarity. Perhaps indeed there is no such thing. Experience shews it is always mingled with greater or less quantities of the *deliquescent salts with earthy bases.* And these are so abundant in some sorts of salt that they render it unfit for the preservation of animal provisions. Beef and even pork, are not guarded by salt so adulterated, from becoming tainted and putrid. That sea salt of this impure quality should be fit for

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curing provisions, it ought to undergo a particular refining operation to rid it of its foreign admixtures. For want of such a process, some sorts of sea salt, though fair to the eye, do not possess an intire and undivided antiseptic power, but so far as the muriate of soda in the mass is alloyed by the middle salts of magnesian and calcarious composition, those parcels of common salt so vitiated become unfit for opposing completely the process of putrefaction. And so far they make a departure from the antiseptic power of pure muriate of soda, the manner of whose action, I endeavoured to investigate in a Memoir addressed to professor Woodhouse and published in the second volume of the New York Medical Repository.

By reason of these foreign and adventitious matters, it happened in Sir John Pringle's experiments, that the common salt employed by him, instead of preventing the corruption of meat, when added in small quantity rather promoted its decay. (Paper III. Exp. 24.) His trials he observes were made with the white or *boiled* salt kept there (in London I suppose he means) for domestic uses. (Appendix to Observations on Diseases of the Army, &c. p. 345. Note.). This kind of salt is known to abound with the *earthy* salts with which ocean water is charged.

Dr. Percival's experiments on sea salt have a tendency to shew that the septic quality ascribed by the learned Baronet to small quantites of common salt is owing to the mixture of *bitter salt* with it. A quantity of this, he observes, adheres to all the common salt used for culinary and dietetic purposes, and as far as its influence goes, it counteracts the wholesome and preservative powers of the clean and unmixed muriate of soda (1 Essays Medical, &c. p. 344,) and that this *septic* quality of the sea salt depended upon the presence of some heterogeneous substance was the opinion of Pringle himself. (Ibid. p. 347.)

Such then being the composition of ocean water, it is easy to explain wherefore it is not fit *by itself*, for washing garments and making them clean. It has a deficiency of *alkaline salt* in it; and alkaline salts are well known to be the most excellent and complete detergents. And it is quite as easy

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easy to assign a reason why it will not answer to employ *soap* with ocean water. The acids united to the lime and magnesia being more strongly attracted by the alkali of the soap, quit their connection with those earths, which fall to the bottom, while the lighter and deserted oil rises to the top. The activity of the alkali of the soap thus overcome by the neutralizing acid of the water, can be of little service, and the disengaged grease immediately thereafter becomes a real impediment.

The basis of all *hard* soap is *soda*. The alkaline matter of *soft* soap is *potash*. This probably happens because the former is prone to *effloresce*, the latter to *deliquesce* in the air. The reason of mingling oil, turpentine and tallow with potash is that this salt is too corrosive to be handled naked or alone. By its causticity *potash* destroys the skin and flesh of the washer, and unless carefully employed, will destroy the goods too. But this is not the case with soda; which in conjunction with carbonic acid may be dissolved in water without exercising any caustic effect upon the arms and fingers of the person who uses it. By virtue of this convenient and excellent quality, the carbonate of soda can not only be used in a lixivial form to cleanse goods, but may be employed to alkalize or soften ocean water and to render it fit for washing with.

It has been ascertained long ago by Professor Home in his experiments on bleaching, that neither sea salt nor any other of the *perfectly* neutral salts composed of an acid and an *alkali* give any *hardness* to water; that the common sorts of sea salt make water *hard* by means only of the heterogeneous salts they retain from the *bittern;* and that *alkalies* by precipitating the earth of salts with an earthy basis and by neutralizing their acids, will *soften* water.

Ocean water, it has been shewn, besides a *perfect* neutral salt, contains a quantity of saline matter with *earthy* bases. To these latter, it owes its *hardness*, or quality to decompound soap. Carbonate of soda decomposes these *terrene* salts and forms with their acids respectively *perfect* neutral salts. The water thereupon becomes *soft*, or in other words, fit for washing goods.

(To be continued.)

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